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CONTENTS OF No. 137.

	PAGE
The Extinction of Species. By Grant Allen	427
The Divided Skirt. By a Lady	428
Dreams. II. By Edward Clodd	429
Pleasant Hours with the Microscope. (*Illus.*) By H. J. Slack.	430
The Universe of Suns. (*Illus.*) By R. A. Proctor	431
International Health Exhibition. III. By J. E. Ady	434
Optical Recreations. (*Illus.*) By F.R.A.S.	436
Electrical Education. By W. Slingo	437
Moved from Afar. III. The Sensations	438
Editorial Gossip	439
Noah's Rainbow. By E. L. Garbett.	440
Correspondence: The Recent Earthquake — Coincidences — Man's Brain—Letters received, &c.	443
Our Mathematical Column: Notes on Euclid's First Book. By R. A. Proctor	444
Our Whist Column: Wonderful Luck at Whist	445
Our Chess Column	446

THE EXTINCTION OF SPECIES.

BY GRANT ALLEN.

ON Lundy Island, in the Bristol Channel, there lingered on till lately, and perhaps there still lingers on even now, almost the last surviving colony of the old British black rat. In the mainland of England itself that aboriginal indigenous rodent is by this time probably all but quite extinct. True, one sees every now and again, among the paragraphs of gossip in the naturalists' journals, a story to the effect that a black rat has been caught and killed by a farm labourer in some out-of-the-way cottage or shieling in the heart of the country; but if the very last specimen has not yet gone finally to his rest beside the last wolf, the last beaver, and the last wild boar, in Mr. Harting's historical collection, it is at any rate quite clear that the black rats have nearly disappeared from the face of the earth, and that only single solitary individuals still drag out a miserable and lonely existence in the holes and corners of this rat-haunted land. Nothing more wretched can be conceived than the position of such last hunted-down representatives of a dying race. Living alone in a world which knows them not; cut off from all social intercourse with their own kind; probably denied the companionship of mates or young—these poor last rats, and last Thames otters, and last Welsh pine martens, have something of the same pathos for the naturalist as ethnologists find in the last of the Tasmanians, dying in the midst of the intrusive white-faces on his lonely rocky Pacific isle.

There is, however, one great point of difference between the extinction of the black rat and the extinction of the wolf and the bear in England, or of the moa, the dodo, and the rhytina or sea-cow elsewhere. These other animals have all been destroyed and annihilated by the hand of man alone, or very nearly so. The wolf, we know, was deliberately hunted down and extirpated in these islands, first in England and Wales, then in Scotland, and finally in Ireland. A similar dead set had probably been made at the British bear at a still earlier period. The moa, in spite of its gigantic size, was eaten down steadily by the first Maori settlers of New Zealand; the dodo was ruthlessly shot off by the early European visitors to Mauritius; and the sea-cow was regarded as a sort of natural larder by the original navigators of the Behring's Straits. Now, extinction of a species in this way could never exactly have occurred before the advent of man upon the world; and therefore it may be regarded to some extent as an exceptional and almost abnormal process of nature. It does not seem probable that any other carnivore except man has ever really succeeded in extinguishing any one of the species upon which it preyed. There appears to be usually a certain natural see-saw between the numbers of the carnivores and of their victims: as the antelopes decrease, the lions necessarily die off of starvation; as the lions die off, the antelopes increase again; as the antelopes grow more numerous, more young lions can find a living for themselves by preying upon them; and so on *ad infinitum*. But it cannot often happen that the carnivore actually kills off an entire species; man alone seems to be cunning enough and persistent enough to carry out a whole system of extinction, deliberate or otherwise, upon that large and regular scale.

In nature at large, before the advent of man, it seems more likely that species generally became extinct by the competition of other closely allied forms. Competition is always keenest and fiercest between those of the same trade. The grocer's deadliest foe is not the soldier or the sailor, but the rival grocer. So it is not the lion that kills out the particular antelope type, but some other and better adapted competing antelope. This is a point which is often misunderstood by the unscientific public in the interpretation of such phrases as "the struggle for existence," and "the survival of the fittest." Strange as it sounds to say so, the worst enemies of each species are not its natural foes, but those of its own household. We could hardly have a better instance of this than the history of the old black rat of England. Throughout the whole of the Middle Ages, the black rat flourished vigorously in our midst, living upon the fat of the land, and having no competitor in its own field within the limits of the British Islands. Now and then, a man might set a trap for a particular black rat, and catch him and kill him; but against the species as a whole, man was practically powerless, for it is a noteworthy fact that while we can easily exterminate a very large animal like the elephant, the bear, or even the wolf, we are unable to cope successfully with smaller creatures like mice or rats, and absolutely helpless to annihilate the Colorado beetle, the common bed-bug, or, still more, the almost microscopic phylloxera. The whole race has been engaged in catching fleas (off and on) for the last hundred centuries, and it is doubtful whether there is one flea the less in the world to-day than there was at the first beginning of that ceaseless crusade. No, it was not man or the dog, his ostensible enemies, that exterminated the black rat; it was an invader of his own kind that eat him finally out of house and home.

Where the brown rat came from nobody exactly knows, but probably from the east, the home of early civilisation, with all its attendant pests. There is some reason to believe, indeed, that the black rat is the older animal of the two—the most primitive house-haunting member of the genus—for it is found even in New Zealand, where it must have been taken over (accidentally, of course) by the ancestors of the Maories, as there are no other indigenous mammals in those great islands. However, the brown rat, developing afterwards its specific peculiarities in India, apparently, acquired a set of qualities which enabled it far better to exist in the midst of a highly-civilised community. For many years it was known in Asia, and, at last, in 1727 (frightened by an earthquake, according to the Russian naturalist, Pallas) it swam the Volga, and invaded Europe.

At once, the old black rat began to find that the new competitor was an enemy of quite another kidney from man or the dog, with their desultory warfare. The big brown rat met the older inhabitant at every point of the compass; competed with him all along the line; eat up everything on which he used to feast in perfect security; and, in short, played toward the black rat the part of the white man toward the Red Indian of America. About the middle of the last century it took ship for England (from Hanover, said the Tory jokers of that day), and, having a most vigorous constitution, with great ferocity, and a wonderful faculty for breeding fast, it soon overran the whole land. The smaller black rat was quickly driven out before it, not, in all probability, by actual hard fighting (though rats are not entirely devoid of cannibal tastes), but by stern competition—the new-comers shortly filling up all the posts in our houses, barns, and sewers, once so securely occupied by the original and old-established British rats. As the brown rat breeds several times in a year, and has sometimes as many as fourteen young in a litter, it is not remarkable that it spreads rapidly wherever it has once acquired a firm foothold.

It is by just such severe competition between closely-allied species and varieties, in all probability, or at least between creatures adapted to fill exactly the same place in nature, that most extinct plants and animals have been really exterminated. One could easily give several other examples of similar extinction taking place beneath our very eyes. I shall be satisfied with recording a single one. Buxbaum's veronica, a handsome, vigorous plant, with comparatively large blue flowers, abundant in Southern Europe, was practically unknown in England some forty years since. More than one local collector has put on record the delight with which he hailed its first appearance in his own garden, much as White of Selborne rejoiced ecstatically over the arrival of the earliest Oriental cockroach in his remote country parish. They would not raise any pæan over either creature at the present day. The southern veronica has rapidly overrun half the gardens in the south of England, till it has become one of our commonest and most persistent weeds; and in doing so, it has ousted the old native *Veronica agrestis*, which is clearly an earlier inferior form of the same type, less healthy, less attractive, and less pushing, with smaller inconspicuous flowers, and a weaker physique in every respect. If Buxbaum's veronica continues to spread with its present aggressive jingoism, it will before long have exterminated its less vigorous competitor entirely, over all the southern portion of these islands. I know many spots where *agrestis* was once common, but where you may now look in vain for any veronica save only Buxbaum's.

THE DIVIDED SKIRT.

By a Lady.

WHEN I wrote, some fifteen months ago, on this subject, I had only worn the divided skirt a few months. I felt even then, however, the good of the change and how much good it promised to do. I can now write after nearly two years' experience, and I think what I have to say may be of use to many.

In the first place I wish strongly to correct the mistaken idea which many have of the divided skirt,—first from pictures in the comic papers, supposed to represent it, and secondly (and chiefly) from the dresses with divided skirts shown at the Health Exhibition. The divided skirt is not and was never meant to be a divided *dress*. It is simply a divided *petticoat*, and practically assumes the form of a pair of trousers with deep frilling. In *this* respect, some of the divided skirts at the Health Exhibition are right enough; but even in their case, every chance of their being ever *generally* adopted by ladies is destroyed by their being only half hidden by the skirt of the dress. They should be fully hidden, precisely as a petticoat would be. In saying this I am expressing no opinion as to the dress being more or less artistic, or more or less hygienic, when the few inches necessary to hide the divided skirt are added to the shortened dress-skirts of Mrs. King. I assert simply, what I am sure to be the case, that ladies never can be persuaded, and ought not to be willing, to wear the dress advocated by Mrs. King and Viscountess Harberton. Mrs. King is doubtless right enough in saying that ladies, and expecially young ladies, do in fact dress with the wish to be, in a sense, conspicuous; there is certainly no harm in a lady desiring to be attractive (which is really what Mrs. King meant) by the elegance of her dress, its grace and style, its well-chosen combinations of colours, and so forth. But not one lady in a thousand will suffer any peculiarity in her dress to appear which will excite ridicule or rude remarks, and the thousandth, who imagines she is helping to introduce improvements in dress when she exposes herself to insult in the streets, is quite mistaken. I believe (from my own experience and the experience of others) that thousands would now be wearing the divided skirt if they had not been deterred by the conspicuous appearance of the arrangement recommended by Mrs. King. (The Turkish trouser form exhibited at the Health Exhibition is simply *impossible.*)

Now to give some results of my own experience.

I first tried the divided skirt for the sake of warmth and lightness. I travelled a great deal, and no matter how many skirts I might wear, I found my lower limbs, from the boot-tops to the knees, suffered always from cold. My husband never suffered in that way, though he had not half so many clothes around him. It seemed to me that I might, without any one noticing the difference, adopt, under the dress (unchanged in length and appearance) the same plan for keeping me warm that he and other gentlemen follow. Mrs. King very kindly let me have one of her divided skirts as a pattern. I made a skirt for myself. I found it just what I had wanted,—warm, light, and comfortable in every way. I was able to leave off all heavy petticoats, and felt at once relieved from a great burden, able to walk farther and more easily, and much more active.

Shortly before this, I had tried the experiment of discarding stays. Like most ladies who have been accustomed to wear stays since girlhood, I found it very difficult to do without them. When I walked my skirts were such a drag—now I had left off stays—that unless I made some change with the petticoats, I must soon have put my stays on again. I was very reluctant to do this, for though I had never laced tightly, or even closely, I felt much freer in many ways, could bend or stoop without fear of whalebones breaking, besides being able (which is perhaps rather unimportant) to *really* breathe,—for the first time (I suppose) since I had begun to wear stays. The change to the divided skirt came to my rescue. With divided skirt and without stays, I was perfectly free. I found, rather to my surprise, that whereas when I wore stays I often felt inclined to depend upon them for support, and so would not always sit or stand quite upright, it now seemed to come naturally to me to hold myself well up. And in passing, I may remark that this is one of the first points which ladies who have left off stays should attend to, if they wish their dresses to

set properly; they cannot expect this if they "double up;" but I can answer for it the fault does not lie with the removal of their stays, though it may have been in part caused by the constant wearing of very tight stays.

In the case of fat persons of the mile-stone persuasion, I know not what to say. I really cannot expect them to go without stays, when I think how they would look at first. But possibly they might find that, relieved of their burden, they would be inclined for more exercise and in that way get a figure considerably better than they now get from stays.

And now, after nearly two years, I can answer with great confidence the assertion, very confidently made by many ladies, that it is impossible to do without stays. At the end of the first week I was convinced that I could do very well without them. Other members of my family who soon after tried the experiment had the same experience. Among the ladies I have persuaded to try it, have been some well advanced in life. With children the effect is delightful, and pleases every one. Nothing would ever induce me (or my friends who have tried the change) to go back to the imprisonment of stays or to resume the burden of petticoats.

DREAMS:

THEIR PLACE IN THE GROWTH OF PRIMITIVE BELIEFS.

BY EDWARD CLODD.

II.

ILLUSTRATIONS of the low intellectual stage of some extant races not quite at the bottom of the scale, drawn from simple matters, will make clearer how they will interpret matters of a more complex order, and interpret them only in one way.

Of the beginning of thought we can know nothing. For numberless ages man was marked out from the animals most nearly allied to him by that power of more readily adapting means to ends which gave him mastery over nature. Through that dim and dateless time he thought without knowing that he thought. "His senses made him conversant only with things externally existing and with his own body, and he transcended his senses only far enough to draw concrete inferences respecting the action of these things." He is human only when the thought reaches us through articulate speech. Language, as a means of communication between him and his fellows, denotes the existence of the social state, the play of the social evolution which gives the impetus to ideas. Language is the outcome of man's social needs and nature; he speaks not so much because he thinks and feels, as because he must perforce tell his thoughts and feelings to others. And by the richness or poverty of his speech we may assess the richness or poverty of his ideas, since language cannot transcend the thought of which it is the vehicle.

By what tones and gestures, by what signs and grimaces, the beginnings of speech were made, we know not. Countless processions of races appeared and vanished before language had reached a stage when the elements of which words are built up could be separated, and the reason which governed the choice of this and that sound or symbol discovered. Now and again, when a correspondence is found between the roots of terms in use amongst the higher races and the names given by lower races to the same thing, we get nearer primitive thought, the correspondence being not always in sound or spelling, which may be delusive, but in physical or sensible meaning. But the inquiry is speculative, and we are concerned with matters of fact.

Language proves the limited range of ideas among barbaric peoples in the absence of their capacity to generalise. They have a word for every familiar thing, sound, and colour, but no word for animal, plant, sound, or colour as abstract terms. It is the concrete, the special shape and feature and action of a thing which strike the senses at the outset; to strip it of these accidents, as we call them, and merge it in the general, and realise its relation to what is common to the class to which it belongs, is an effort of which the untutored mind is incapable. Many of the northern non-Aryan tribes, as among the Mongols, have names for the smallest rivulet, but no word for river; names for each finger, but no word for finger. The Society Islanders have a separate name for the tails of various animals, but no name for tail. The Mohicans have verbs for every kind of cutting, but no verb "to cut." The Australians and other southern aborigines have no generic term for tree, neither have the Malays, yet they have words for the several parts—the root, stem, twig, &c. When the Tasmanians wished to express qualities of things, as hard, soft, warm, long, round, they would say for hard, "like a stone"; for tall, "long legs"; for round, "like the moon," and so on. Certain hill-tribes of India give names to sunshine, candle, and flames of fire, but "light" is a high abstraction which they are unable to grasp. Some of the Red Race languages have separate verbs for "I wish to eat meat," or "I wish to eat soup," but no verb for "I wish." Of course, the verb "to be," which, as Adam Smith remarked long ago, is the most abstract and metaphysical of all words, and therefore of no early coinage, is absent from a large number of barbaric languages. Abstract though it be, it is, as Prof. Whitney points out, made up of the relics of several verbs which once had, like all elements and parts of speech, a distinct physical meaning. As in "be" and "been" the idea of "growing" is contained, so in "am," "art," "is," and "are" the idea of "sitting" (or, as some think, of "breathing") is embodied. As an example of its absence, the Abipones cannot say "I am an Abipone," only "I Abipone." Turning to another class of illustration, we have proof what a far cry it is from the savage to the Senior Wrangler in the powerlessness of the former to count beyond his fingers—indeed, he cannot always count as far as that, any number beyond two bewildering him. One of the best stories to the point is given in Mr. Galton's "Tropical South Africa."

"When the Dammaras wish to express four, they take to their fingers, which are to them as formidable instruments of calculation as a sliding rule is to an English schoolboy. They puzzle very much after five, because no spare hand remains to grasp and secure the fingers that are required for units. Yet they seldom lose oxen; the way in which they discover the loss of one is not by the number of the herd being diminished, but by the absence of a face they know. When bartering is going on, each sheep must be paid for separately. Thus, suppose two sticks of tobacco to be the rate of exchange for one sheep, it would sorely puzzle a Dammara to take two sheep and give him four sticks. I have done so, and seen a man put two of the sticks apart, and take a sight over them at one of the sheep he was about to sell. Having satisfied himself that that one was honestly paid for, and finding to his surprise that exactly two sticks remained in hand to settle the account for the other sheep, he would be afflicted with doubts; the transaction seemed to come out too "pat" to be correct, and he would refer back to the first couple of sticks; and then his mind got hazy and confused, and wandered from one sheep to the other, and he broke off the transaction until two sticks were put into his hand and one sheep driven away,

and then the other two sticks given him, and the second sheep driven away. Once while I watched a Dammara floundering hopelessly in a calculation on one side of me, I observed Dinah, my spaniel, equally embarrassed on the other. She was overlooking half a dozen of her new-born puppies, which had been removed two or three times from her, and her anxiety was excessive, as she tried to find out if they were all present, or if any were still missing. She kept puzzling and running her eyes over them, backwards and forwards, but could not satisfy herself. She evidently had a vague notion of counting, but the figure was too large for her brain. Taking the two as they stood, dog and Dammara, the comparison reflected no great honour on the man."

Dr. Rae says that if an Esquimaux is asked the number of his children, he is generally puzzled. After counting some time on his fingers he will probably consult his wife, and the two often differ, even though they may not have more than four or five in family. Of the languages of the Australian savages, who are the lowest extant, examined by Mr. Crawfurd, thirty were found to have no number beyond four, all beyond this being spoken of as "many," whilst the Brazilian Indians got confused in trying to reckon beyond three. The list of such cases might be largely extended, and although exceptions occur where savages are found with a fairly wide range of numbers, notably where barter prevails, the larger proportion of uncivilised people are bewildered at any effort to count beyond three or five. The fingers have, in most cases, determined the limits, for men counted on these before they gave words to the numbers, the words being at last borrowed from the fingers, as in our "five," which is cognate with the Greek "pente," and the Persian "pendji" (said to be derived from the word for "hand"), and "digits," from Latin "digitus," a finger. This limited power of numeration thus shown to be possessed by the savage, justifies the remark made in a former paper, that he is nearer to the ape than to the average civilised man, nearer, as the extract from Mr. Galton shows, to the spaniel than to the mathematician. What conception of the succession of time, still less of it as a confluence of the eternities, can he have whose feeble brain cannot grasp a to-morrow! And yet the difference is not one of kind, but of degree, which separates the aborigines of Victoria or Papua from the astronomer who is led by certain irregularities in the motion of a planet to calculate the position of the disturbing cause, and thereby to discover it nearly a thousand million miles beyond in the planet Neptune.

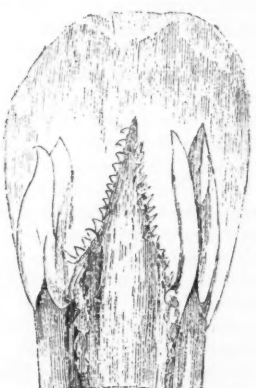
PLEASANT HOURS WITH THE MICROSCOPE.

BY HENRY J. SLACK, F.G.S., F.R.M.S.

AT this time of the year there are numbers of interesting facts to be observed in the structure of various garden and field flowers, and it is well to walk about with a pocket magnifier, and examine all that are found in bloom. Look, for example, at an Azalea or a Rhododendron. Both belong to the Heath family, and the anthers of both can be seen with a low power to be formed like little bottles with open mouths, out of which comes the pollen when it is ripe. Many insects frequent the rhododendrons, and big ones, such as humble-bees, rout its flowers about very roughly. They can, however, do no harm to the pollen while it is maturing inside the bottle, and when it is ripe they carry it off on their hairs, and convey it to another flower on the same or on another bush.

The pretty Borage, so striking for its exquisite blue tint, and its relative, the Comfrey (*Symphytum*), exhibit other curious arrangements. The stamens of the Borage press round the pistil, enclosing it as in a tube. Down below, at the base of the stamens, is the honey-drop the bees wish to get at; and, to do this, they must push the anthers on one side, as they thrust in their tongues. The pollen, when ripe, is discharged inwardly, and falls into the space between the filaments and the style. The bee's tongue comes into contact with it, and carries it away. Then the insect retires, the anthers spring back to the style, and the pollen is again shut up, and safe from other attacks. Bees, on any one excursion, usually proceed from flower to flower of the same sort, and many insects only frequent particular flowers, so that when any kind of pollen adheres to them, it is likely to be taken to a plant of the same species. There are numerous ways in which self-fertilisation is avoided, and the pollen of one flower conveyed to another by the agency of insects, or by the wind. In the latter case, the supply of pollen is large enough to allow for much loss, and the pistil is feathered or sticky, so as to have a better chance of catching some of it.

By the way side and in the meadows there is now abundance of sorrel (*Rumex acetosa*). It often supplies a pretty red tint to grass fields that contrasts well with their green; but the ordinary wayfarer or tourist, though he may chew a leaf or stalf for its refreshing acid, is heedless of its beauty. A hand-magnifier shows that elegant little tufts of pink and white hue and glassy aspect protrude from its imperfect flowers. They are like sea polypes with their tentacles displayed, and are fringed appendages to the three short styles. The flowers are generally dicœcious, the two sexes being on separate plants, but sometimes they are monœcious and diclinous, on the same plant, but on different flowers.



Segment of Comfrey Petal: two anthers and one scale. Tip of segment recurved. × about 12.

London Pride, now in free bloom, is a very fine object for the microscope and a low magnification. The white petals are elegantly decorated with yellow spots below groups of pink ones. The stigmas are supported upon a round cushion, from beneath which the anther filaments radiate in star pattern. The stems are covered with glandular hairs, like minikin pins with ruby heads. The plant belongs to the Saxifrage family (*Saxifraga umbrosa*), and besides its London appellation is called None-so-Pretty, and, for some reason a student of folklore may be able to explain, St. Patrick's Cabbage.

To return to the Comfrey, we find its flowers in pendulous groups (*exuves*). Each flower hangs from a pedicel, and its tubular corolla is five-cleft. The humble-bees are very fond of it, and pierce holes near the base of the corolla to get out some nectar-juice. Looking over a considerable number of flowers I found all with two or more perforations, often so close as to be nearly continuous. Is this done because the juice of the corolla is what the bees want? In other cases, as in the flower of the arbutus, the insects generally content themselves with one hole, and the hive-bee avails itself of the opening made by his bigger and stronger-mandibled relative. Humble-bees do not mind being closely watched, and will allow a hand-magnifier to be brought near them without disturbing their proceedings. In many instances I noticed that they thrust their long tongues into the mouth of the flower as well as into the lower part of the corolla. This brought them into contact with its pollen.

Portion of Borage-flower, showing the position of anthers. × about 12.

The annexed sketches show a portion of the Comfrey corolla split open and flattened. Next to the anthers are ornamental scales, with glandular bulbs at their edges. These group themselves round the style, which is long, and projects beyond the corolla. In the common Borage, to which the bees' visits have been described, the style is shorter than in Comfrey, and the stigma is hidden by the anther group. The five stamens have short-forked filaments, and the anthers are dark purple, almost black. The narrow sepals, which are of a brown tint, add five intermediate rays to the broader-rayed star of the five petals, which they alternate with. Each petal of this flower has a glassy-looking scale at its base, composed of glittering cells, and with five hairs inside. The five scales form a cup, and next to them are dark purple, finger-like projections, shorter than the anthers. The Borage family may be generally known from all but the Labiates—which they in no other respect resemble—by their ovaries presenting the appearance of four little nuts.

Besides the arrangement mentioned, which facilitates the fertilisation of the flowers by bees, the opposite practice of self-fertilisation is hindered by the flower being *protandrous*, that is, having the anthers ripened before the stigma. As the Borage is in flower a long time, the bees, in passing from one plant to another, and from one flower to another, are pretty sure to find ripe pollen in some and stigmas fit to receive it in others. Lungwort (*Pulmonaria*) is a Borage which flowers in the spring, and is recognisable now by its pretty spotted leaves. It has quite a different method of obtaining cross-fertilisation. Its nectar is very attractive, and it produces two sorts of flower, one with long styles and the other with short. Insects carry the pollen of one form to the stigma of the other, and it was supposed no seed could be produced otherwise; but Lubbock cites Darwin as having raised seedlings from long-styled plants which were fertilised by their own pollen.

Pig-nuts are now abundant in grass-fields, road-sides, and gardens. They may be known by their long, thin stems, from one to two feet high, slender leaves, and heads of very small white flowers. They belong to the umbeliferous or parsley family, and get little notice except from schoolboys, who dig up their edible roots; but they deserve the attention of the microscopist, as their flowers are beautiful little things, and both in their partially and fully open states, suggest patterns for the designer.

The above plants are mentioned merely as specimens to excite attention. It may be added that such objects as the stigma-fringes of the sorrel may be mounted in Canada Balsam, well thinned with benzine, having first soaked them for a few minutes in carbolic acid; the colour is lost, but the form well preserved.

THE UNIVERSE OF SUNS.

By RICHARD A. PROCTOR.

THE method of star-gauging imagined such a degree of uniformity in stellar distribution within our star system,—assumed to have attainable limits—that the number of stars seen within any given field indicates fairly the extension of the system in the direction of that field. Taking fields equal in size and observing them with equal telescopic light-gathering power, we should have the same sort of estimate of extension that we should obtain of sea depth by letting down a tube to the sea-bottom and weighing all the water inclosed within the tube. All that is necessary to make the estimate satisfactory, is that we should really reach the limits of the system, and that the stars really should be spread with tolerable uniformity. The size of individual stars counts for nothing in this method, assuming only (as Herschel assumed) that a telescope powerful enough to reach easily to the limits of the system, would show every star within the system; in other words, that there are no *very* great discrepancies of magnitude.

It was as the result of this process and no other, that Herschel arrived at the stratum-theory of the sidereal system—or rather the cloven-stratum theory.

When this theory was formed, Herschel had not met with even the simplest cases of unequal star distribution; he had not yet learned that such objects as binary stars exist in the universe. Struve has well pointed out that the discovery of the physical association between certain double stars changed the whole aspect of the star system in Herschel's eyes. From this time he recognised the fact that there are different orders of stars, and different systems of star-grouping.

Herschel himself abandoned altogether, as well the principle of star-gauging as the results to which it had led him.

Herschel not only said that the Milky Way was not what he had supposed; but he stated what in his opinion was the actual shape and constitution of certain parts of the Milky Way. He found that certain of the brighter

parts of the Milky Way were not regions of the sidereal system owing their apparent richness to their vast extension, but true clustering aggregations—not parts of an extended stratum but globe shaped.

The *resolution-test* is very commonly confounded with star-gauging, but is quite distinct in principle. According to this plan the distance of any star-group was to be determined by the telescopic power necessary to resolve the group. To show the distinction between this method and the other, it is only necessary to point out that in star-gauging, the same telescope, or at least the same telescopic power, was necessarily employed throughout the inquiry; in the new method, the telescopic power was made variable, gradually increasing power being applied until a star-group was completely resolved, and the power required for this purpose was held to indicate the distance of the group. There was a partial return, in the adoption of this method, to the general idea of uniformity of distribution which had led Herschel to erroneous results in 1785; and in precise correspondence with this fault in the new method we recognise in the results obtained an obvious incorrectness, which would not have escaped the attention of Herschel in earlier years.

The new method was one to be tested, not *adopted*. The method of star-gauging had been adopted, and had eventually to be rejected. This experience should have suggested that no new method ought to be adopted until it had been subjected to some adequate test. The results obtained by Herschel showed sufficiently (and in his earlier years would have been regarded as showing) that the method was unsound.

Let one point suffice to show this :—

The fine double-cluster in the sword hand of Perseus was partly resolved with the lowest power Hershel employed, and this, according to the criterion he had adopted, showed that part of the cluster lay only just beyond the limits of naked-eye vision. But he was unable with the highest powers he employed to resolve this spot completely, and this, according to his criterion, showed that part of the cluster lay beyond the range of his highest powers, or some twenty times farther away than the limits of naked-eye vision. Thus, according to his criterion, the distances of the nearest and farthest parts of the cluster are as one to twenty, at least. But the apparent size of the cluster shows that the breadth of any part of the cluster is less than the hundredth part of the distance of that part. Combining these indications, we have for the shape of the space within which the cluster is included, something like A B (Fig. 8), where S is the place of the Solar system.

Fig. 8.

But no one can accept so monstrous a result as this; nor would Herschel for a moment have adopted it, had he noticed that his own figures led directly to it. Apart from all other objections, the circumstance that this long-projecting cluster had the Sun directly at its apex, would have sufficed to show that some other interpretation must be adopted. But no one who has observed this or any other star cluster can doubt for an instant that the portion of space occupied by the cluster is rounded—speaking roughly. The cluster is not a globular cluster, of course; it has outlying branches and streams (and appears, by the way, to be connected with the general stream of the Milky Way in this neighbourhood); but that, regarding it as a whole, we must consider it as a rounded nodule or double nodule of the galactic stream, and not as a projecting region directed exactly from the sun, no one can doubt who has ever examined the cluster.

Nor did Herschel question this, or fail to recognise the soundness of the method of reasoning, illustrated by Fig. 8, which I have used to dispose of the strange result to which Herschel's figures lead. But the real fact is that Herschel did not live to examine fully the facts he supposed he had accumulated in and after 1817. He did not do much, indeed, towards carrying out the scheme which he had indicated. The papers of 1817 and 1818 may be compared respectively with those of 1784 and 1785; and I venture to say, that if in 1818 Herschel had had before him as long an observing career as in 1785, we should have found him definitely abandoning the principle he indicated in 1817, precisely as we find him in 1802 definitely abandoning the principle he indicated in 1784.

The fact is, both principles were matters to be tested, not to be adopted as rules for guidance.

William Struve adopted a singular combination of the two methods, and interpreted the results he obtained in a most remarkable, and as I think, altogether unsatisfactory, manner.

Struve perceived that if the principle of star-gauging had been sound, the stars of the brighter orders ought to be strewn with general uniformity over the heavens; for the range of naked eye vision, or low telescopic power, for separate stars lies far within the limits of the stellar system, even in its narrowest parts—that is, measured in the direction of its thickness. He saw then that here was a new test applicable to the star-gauging principle. It was only necessary to take some complete catalogue of stars down to, say, the 7th or 8th magnitude, and to try whether the stars grew richer as the Milky Way was approached. If they did grow richer, then it would be manifest that the principle of star-gauging could not be trusted *in all its generality*. (The reader will presently see why I emphasise these four last words.)

Struve took a catalogue complete only for an equatorial zone thirty degree wide, and extending fifteen degrees from the equator on the north and on the south. The catalogue was tolerably complete down to the ninth magnitude inclusive. Like other catalogues, it was numbered and arranged in order of Right Ascension; and what Struve did was simply to ascertain how many stars it included in the different "hours." He found that the Milky Way "hours," the seventh and nineteenth, were the richest, and that there was a gradual increase up to the maximum of richness, and then a gradual decrease down to the minimum, so that there was all the evidence his method could afford to show that the nine brighter order of stars do increase in numbers as the Milky Way is approached.

If he had been satisfied with this result, Struve would simply have obtained rough evidence of those laws of stellar distribution which my methods have more particularly indicated.

But he was not so satisfied, and it was in analysing his result that he applied the novel considerations I have spoken of.

He first assumed that the law of distribution for an equatorial zone thirty degrees wide, might be regarded as the law of distribution for the circuit of the equator itself —a particularly daring assumption, when we remember that the galaxy does not cross the equator at right angles but slantwise, so that an "hour" of stars thirty degrees in declination range crosses parts of the star sphere of very different richness (assuming, as Struve did, that the mid-zone of the galaxy is the zone of greatest richness). This first step then—this substitution of the equator itself for an equatorial zone covering more than a quarter of the whole heavens—was directly calculated to mask the real features of stellar distribution.

But the next step was even more daring. Having found that there is a variation of star-richness round the equator, two opposite points of which circle mark the place of maximum richness, Struve next changed the circle into a disc, and in this way:—

Suppose A B, Fig. 9, to be one "hour" of the celestial equator, S the place of the solar system, *a b c d* one "hour" of the equatorial zone dealt with by Struve. Then he took the stars within the space *a b c d*, and having distributed them uniformly along the arc A B, he next spread them

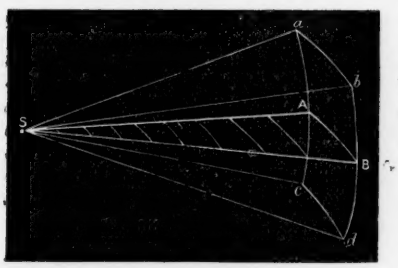


Fig. 9.

(according to the numerical distribution of their different orders of brightness) over the sectorial area A B S, Figs. 9 and 10.

Surely an amazing application of the laws of averages, to take the stars spread over the area *a b c d* of the celestial sphere, where A B is an arc of fifteen degrees, and *a b*, *c d*

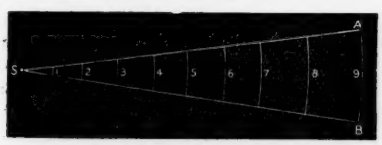


Fig. 10.

each of thirty degrees, and to conceive these stars spread, according to a special law of uniformity, over the sectorial plane A B S. Yet it was thus that Struve's celebrated *section* was obtained.

I hesitate to point out objections against this process, because I cannot conceive what arguments can possibly be urged in its favour.

Struve himself considered that his result agreed perfectly with the views of Herschel in 1817 and 1818; and it will be perceived that there is something in the formation of the sectorial star-area S A B, which bears a distant resemblance to Herschel's resolution test of star-distance. On the other hand, Struve perceived that his results were altogether opposed to the general theory of star gauging. He adopted, however, a modified form of the principle, since he assumed a very peculiar law of uniformity of distribution.

[In passing, I may remark that, in answering the first paper I read on this subject at the Royal Astronomical Society, Professor Pritchard made the somewhat remarkable statement that all Herschel's results confirmed each other, and that Struve's agreed with them. But it is certain that Professor Pritchard has never read Sir W. Herschel's papers through.]

The plan on which I have proceeded has been unlike any of those hitherto described, and the principle I have adopted is not an assumption, but one whose justice depends on manifest and sufficient considerations.

I have, in the first place, set equal surface charting as the only effective way of indicating laws of distribution, if any such exist; and charting *generally* as a necessary substitute for mere cataloguing, or other process of numerical estimation, whether as applied to the stars of various orders, to nebulæ, to clusters, to double, coloured, or variable stars, or lastly, to star motions. You can *see* a chart, study its relations, return again and again to its investigation, compare it with other charts, or combine it with them in a new chart, and so on. Any characteristics which charts are competent to exhibit, they show to all, and reveal independently of any special process. The case is altogether different with catalogues. Their capacity for concealing the truths really contained in them, and for mystifying the student of nature, and their adaptability for deceptive processes of manipulation, have been repeatedly illustrated in scientific researches.

But of course, when once a chart has been drawn, it suggests questions for statistical research; or rather it suggests really effective methods of dealing with numerical relations. And the construction of charts of distribution, has led me to notice two principles of analysis, which are manifestly sound, and have already been shown to be effective, while I believe very confidently that they will hereafter be found to be the only principles which can enable us to make any satisfactory advance in dealing with those portions of space which lie beyond our means of measurement.

They are these:—

A. If objects of one class are found to be spread *over certain parts of the celestial sphere* more richly than over others, and if objects of another class are found to exhibit corresponding peculiarities of distribution, being always more richly spread or more sparsely strewn where the same is observed with objects of the former class, then the two classes of objects form one system, and are intermixed within that system, the same *subordinate* region of *space* within that system including the aggregations of both classes of objects.

B. If, on the contrary, a law of contrast is observed in the distribution of objects of two different classes, so that the distribution of the objects of one class is *systematically* richer or poorer according as the distribution of objects of the other class is poorer or richer; then the two orders of objects form one system, but are separated from each other within that system, those subordinate regions of space which include many objects of one class having few or no objects of the other class, and *vice versâ*.

I will illustrate these principles before passing on to a third, which is only *probable* (not as these are *certain*,—where only the evidence described can be obtained in sufficient *amount*)

Suppose the whole sky were covered more or less richly with flying creatures; suppose it could be seen that there were two well-marked classes of these creatures, large ones and small ones: then if, wherever the large ones were

richly strewn over *the sky*, the small ones were likewise so strewn, while where there were few large ones there were few small ones, it would be a safe inference that the large and small ones were intermixed in the *aërial spaces*, travelling together in flights or groups. They might be old birds and their small youngsters, or birds pursuing their prey among flights of insects, or the like; but whatever the two classes might be, it would be certain (supposing always that the peculiarity in question were sufficiently marked) that the large and small objects were intermixed.

But, suppose the reverse held, and that where there were many large flying creatures, there were few or no small ones, and *vice versâ*, the peculiarity being very marked. Then it would be manifest that in this case also there was a law of association, but one which caused the objects of the two classes to separate into different groups. The large objects might be birds of prey, and the smaller might be weaker birds, banding together for mutual protection, or the like; but it would be certain that the peculiar law of arrangement observed among the flying creatures was due to some real relation between the two classes, this relation causing them to keep apart from each other.

In neither case (and let the reader notice specially, that what I am about to say applies equally to both cases), in neither case could we imagine for a moment that the large flying creatures were of the same class as the small ones, but much nearer; for it would be practically impossible that by some strange accident two strata of creatures, flying at different levels, should have their rich and poor regions so adjusted as to be seen, in all cases, in the same direction, by the observer on earth.

Thus we are led to the third principle, which only affords probable evidence, though, in some cases, such evidence may approach very nearly to demonstration.

C. If objects of two classes are spread over certain parts of the heavens more richly than over others, but the rich regions of one agree systematically neither with the rich nor with the poor regions of the other, then the probability is, that the two orders of objects lie at different distances, and are in no way connected with each other.

This probability rises into certainty where some markedly peculiar arrangement of one order is seen to be carried across some equally marked arrangement of the other, the two arrangements being manifestly independent of each other.

The principles thus enunciated, are those which I have followed in the interpretation of all my equal surface charts, whether of stars or nebulæ. Applied to my chart of 324,000 stars, principle A shows conclusively that neither the earlier nor later view of Sir W. Herschel, respecting the Milky Way, nor the theory of the elder Struve, is even an approach to the true theory; *but*, the *general* views indicated by Sir W. Herschel during the period of his observing career intermediate between the abandonment of the star-gauging principle and the adoption of the resolution-test, accord fairly with the observed facts. The point overlooked by him, by Struve, and by all other astronomers, except the younger Herschel (who, however, did not sufficiently dwell on its importance), is the evidence of the intermixture of stars of many orders of real magnitude within the same regions of space. And as principle A, applied to the teachings of my chart of 324,000 stars, shows the Milky Way to consist of a system of star-streams, including many orders of stars, from the highest down to what must be regarded relatively as mere star-dust, so principle B, applied to my charts of Nebulæ (and still more clearly when applied to the exact charts recently submitted by Mr. Sidney Walters to the Astronomical Society), shows that the star-cloudlets belong to the same system as the stars of the Galaxy, but are so distributed within that system as to be rich where stars are relatively few, and few where stars are relatively rich.

But much wider and more systematic research is still required. It will be manifest that the three principles, but especially the first, admit of being applied most effectively to stellar research—much more effectively than star-gauging as originally proposed, even if it had turned out that the principle of such star-gauging was sound. By applying telescopes of different power to systematic star-gauging, as distinguished from the mere selection of a few widely-scattered fields, and by employing for each survey only dark, clear, and moonless nights, instead of using (as was the case with Herschel's surveys) moonlight, twilight, and hazy nights, and even daylight, an estimate can be formed of the laws of distribution of the stars of various orders, proceeding onwards from those included in my chart of 324,000 stars, and thus extending and amplifying the teaching commenced in that chart. When that work has been accomplished, we shall begin to understand the real wonders of the star depths, the magnificence of the subordinate star-schemes which have been mistaken for the sidereal system itself, and something of the grandeur of that system, whose limits lie far beyond the range of our most powerful telescopes, while within them are included all the various orders of celestial objects which the telescope has yet revealed. Combining such lessons with what has been learned and yet remains to be learned of the movements taking place within the sidereal universe, we shall have a picture grander and more impressive by far than any yet presented to our contemplation; we shall learn that the true galaxy is infinitely more extended, infinitely more complex in structure, than we have supposed, and that the processes at work within its bounds are infinitely more stupendous.

THE INTERNATIONAL HEALTH EXHIBITION.

BY JOHN ERNEST ADY.

III.

THE subject-matter of our present report will be confined to a general review of one of the most striking series of exhibits which it has been our good fortune to inspect. Within the space called the Central Gallery, at its eastern extremity, a rectangular space has been allotted to Messrs. Doulton & Co., of the Lambeth Sanitary Engineering Works and Art Potteries. So judiciously have they utilised the area entrusted to them, that visitors to South Kensington will find it well worth their while to spend a few hours there to admire and to learn.

We propose to give an outline of what might with justice be called an exhibition of itself; for therein are to be found works of art which would furnish material for a library of treatises, harmoniously blended with the requirements of sanitary science. We are allured into a realm where all is lovely in form and beautiful in hue, and where the watchword at the portals is "health." We leave the Doulton trophy and its annexes with the inward satisfaction of having gleaned a vast amount of profitable information, in an easy and pleasant manner. Our lingering glances dwell upon the tiers of filters we have examined, and remind us that we are soon to attempt the mysteries of the "water supply question," a theme which deservedly takes the foremost place here, as bearing directly and most powerfully upon the sanitary condition of humanity at large.

The "Doulton Section," as we shall term it, may be approached from the East Gallery down a short flight of stairs, and most persons would be inclined to hurry on to view the details of the mosque-like edifice before them, which occupies the central portion of the pavilion; but we would divert their attention for a moment toe-wards. They there stand on ground of adamant, for the terra-cotta steps are fitted with the patent so-called "Silicon Tread," which is formed of a material more durable that the hardest of Nature's stone, and has been expressly placed in its present frequently traversed position in demonstration of its quality to resist wear and tear.

One more interruption and we shall go *in medias res* without further comment. To the left and right hand side of the descended staircase are two exhibits of vases on shelves, one on each side against their respective walls. Those to the left-hand side are peculiar to the Lambeth Art Potteries, and are known as the famous "Doulton ware." They are the result of a prize competition by assistants in training. The original decorative designs have been produced upon a piece of ware of previously unknown shape by the unaided skill of the students, within a limited period of four hours. We do not like to make unfavourable comments, but we cannot here refrain from thinking that this *time* plan is essentially pernicious to the cultivation of legitimate art; it is somewhat analogous to the wretched system known as "cram" amongst science teachers; pupils are apt to coach up striking designs which can be

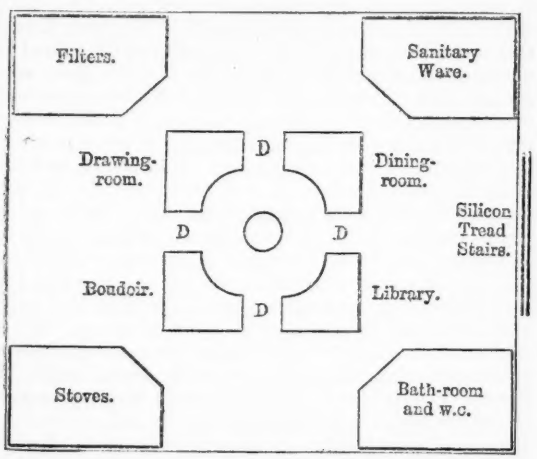


Fig. 5.—Plan of Messrs. Doulton's Pavilion.

executed in a short space of time, and finally veiled plagiarism takes the place of originality. Of course, from a purely commercial point of view, things are entirely different; nevertheless, the vases are excellent examples of the ware they are intended to illustrate. The glazes used are exceptionally pure and clear, and what seems to us to be an important advance from an artistic point of view, in Doulton ware, is, that although the colours chosen are brilliant, yet they are so happily intermingled as to leave in their totality a pleasurable impression rather than a sense of gaudy vulgarity. One can imagine a place for these ornaments in a room laid with Eastern carpets and lighted through delicately stained window frames. We were also led to observe that their glazes of splendent colours are free from unsightly cracks, or, as potters term it, they do not "crase." This crasing is a kind of bugbear to the potter who endeavours to produce rich hues: but Doulton ware evidently gets "scot free," probably because the coat of glaze penetrates into the "body" of their stone ware.

On the right-hand side of the terra-cotta Silicon-tread staircase there are a number of stunted urns of "Silicon ware," also the result of competition. These vases are unglazed, and although the colours are bright and pure, they are subdued, so that a singularly softened effect is produced. They are well worthy of careful examination, and do great credit to the students, especially to the lady students, whose names predominate among the prize-winners.

The general disposition of the exhibits may be gathered from a glance at the subjoined figure (Fig 5). A central square, access to the interior of which may be gained by four median, diametrically opposite, arched doorways (D. Fig. 5) gives support, upon elaborately-decorated columns of Doulton ware, to a graceful dome of about 30 ft. in height. This structure is almost unique in this country, and is based upon the Indian principle of dome architecture, where the arches of which it is formed are halved into every alternate arch, thereby securing a maximum of strength with a minimum of material. The superstructure is covered with tiles of coloured and glazed terra-cotta, beneath which an external facing of panels of mosaic in the new Silicon ware alternates with stained-glass windows.

The external decorations of the lower portion of the pavilion consist of sixteen elaborate panels of hand-painted tiles, each one of which is a masterpiece, and tells a tale of its own. An exhaustive description of these pictures would involve a labour so considerable, that these pages, magnified an hundredfold, could not contain a tithe of what might be said. They recall the patient experiments of generations of philosophers, artists, and workmen, and lead back even to the mystic ages of the Celestial Empire; they would necessitate an explanation of the vast subject of the rise and progress of the potter's art in all its intricate phases, with biographies of the pioneers who may be said to mark each epoch in its history. Palissy, Della Robbia, Wedgewood, Shon-Sui, all stand forth in living pictures to attest to the skill of their descendants—the Messrs. Doulton. Then we are brought face to face with pictures of old London, the "Bishop's Walk," "High-street, Lambeth," "Lambeth Palace," and, most modern and important of all, "The Albert Embankment," and birthplace of this magnificent pavilion. A brief titular description of the pictures —and, indeed, of the entire exhibit—may be obtained at the pavilion by asking for it. Around the columns, which support the dome, are grouped four small rooms, fitted respectively as a drawing-room, dining-room, library, and boudoir.

The annexed corners of the space occupied by Messrs. Doulton are devoted to the display of their recent inventions and improvements of articles which come more emphatically under the title of health exhibits. Several of the appliances shown are of such great importance that we shall devote our next communication to their consideration. They deal with drain-pipes, sanitary appliances, stoves, and filters; and although the names of these things conjure up a host of disagreeable fancies, we may assure our readers that an account of some of the ins-and-outs of these necessaries will not only be interesting, but of great practical value, and highly delectable, especially when it is borne in mind that they are the outcome of a toil which seeks to cope with the evils of modern civilisation, and to establish sanitary reform.

AN electric light installation has been completed at the Park Pit Ocean Collieries, South Wales. A 6-horse-power engine drives a Crompton-Bürgin self-regulating dynamo. Fifty 20-candle power Swan incandescent lamps are distributed underground and on the surface over the screens, workshops, and engine-houses.

OPTICAL RECREATIONS.

BY A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

(*Continued from p.* 391.)

IN the various devices for popularly illustrating the effects of reflection that we have so far described, we have assumed such reflection to take place from a silvered mirror or mirrors; but the surface of unsilvered glass reflects light too, and does so while other rays are passing through it from any object behind it. This supplies us with the means of producing that effective optical illusion so well known in this country as "Pepper's Ghost." Fig. 9 exhibits a mode by which this can be shown upon a drawing-room table.

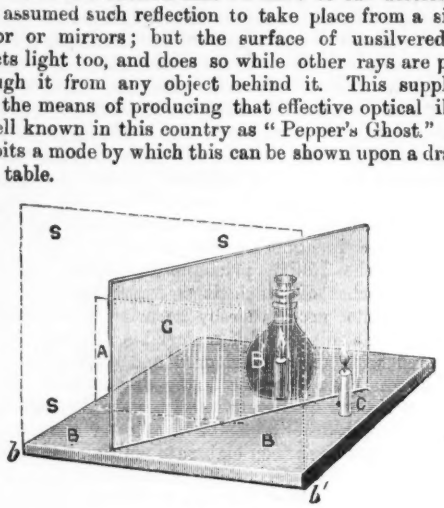


Fig. 9.

B in the figure above represents a slab of inch board, two feet long and one foot wide, across which, in a groove, fits tightly the sheet of carefully-cleaned plate-glass, G, at an angle of 45° with the length of the board. This should be fixed accurately upright. At B′, behind it, we place a tall clear glass-bottle full of water, and in front of the glass, at C, a candle, which we shift about until its image, reflected in the glass, seems to fall in the middle of the water-bottle. A millboard screen, S, covered with black paper, and having a central rectangular aperture, A, should, prior to the exhibition, be fastened to the front edge, *b*, *b′* of the board, B, so as to hide the candle. The uninitiated spectator, seated at a suitable distance, and looking through the aperture, A, will imagine that he is viewing a candle burning in the very middle of a bottle-full of water. Numbers who will read these lines must have witnessed the spectral forms produced at the old Polytechnic Institution by a slight modification of this device on a larger scale.

The principle of reflecting rays from plane mirrors, at right angles to their original direction, has been utilised (among other ways) in the observation of an enemy from behind earthworks, where exposure in the embrasures was rendered dangerous by opposing marksmen. A pretty application of it, too, is seen in the capital conjuring trick of the Sphinx, in which a human head delivers oracular responses from a table on which it lies, the table being a tripod one, without any cloth on it, and (seemingly) absolutely void and detached from all surrounding objects, save the floor beneath. Enough, though, has probably been said on the subject of reflection from a plane. Let us now try to ascertain what happens when light falls upon a curved surface. Upon p. 390 we enunciated the law that "when a ray of light is incident upon a reflecting surface, the reflected ray is in the same plane with the incident ray, and the angles of incidence and reflection are equal;" and this we have shown to hold rigidly true where such reflecting surface is a plane. What, though, will happen if, instead of a plane surface, rays of light impinge upon a curved one? Will our law still hold good? It will. Let us take a concave mirror as an examp'e, and track the course of the rays falling upon it from a luminous point in front of it.

Fig. 10.

We may consider a curved mirror as made up of an infinite number of little planes, which are called, mathematically, its "elements;" and it is quite evident that a radius of curvature, drawn from the centre of that curvature to any point in the mirror, is perpendicular, or square to its surface at that point. Thus, in our figure above, Cp, Cp^1, Cp^2 are all perpendiculars to the mirror at the points p, p^1, and p^2; and, from what has preceded, it will be evident that light reaching the mirror in these lines will be reflected back along them to the centre of curvature C, whence the light must have emanated. Suppose, though, that instead of our luminous object being on the axis of the mirror at C, we remove it to A; let us trace, at an equal distance from the mirror, the course of the rays emanating from it under these conditions. Here, Ap is the incident ray; and C being the perpendicular at its point of incidence, the angle ApC will be the angle of incidence, to which BpC, the angle of reflection, will be equal. In the same way, Ap^1C and Bp^1C are equal, as are Ap^2C, Bp^2C—and so on; so that an eye at E, 10 inches or so from B, will see an image of the luminous object A, accurately reproduced in form, size, and colour at B, though inverted; or such image may be received upon a screen placed at that point. This is technically spoken of as a "Real image," in contradistinction to the "Virtual image" of an object produced by a plane or convex mirror, or by an object within the focus of a concave one. It will serve to test the progress of the student up to this point if he will construct a diagram for himself of a concave mirror, drawing as above radii of its curvature for perpendiculars, and ascertain what will be the course of parallel rays—or those from an infinite distance—falling upon it. He will find, as a matter of fact, that they will all meet at a point halfway between the middle of the mirror's surface and its centre of curvature. This point is called the "principal focus" of the mirror. Moreover, he will see that the rays from a luminous point situated in the principal focus will be reflected from the mirror in lines rigidly parallel to its axis. Concave mirrors are, unfortunately, not very cheap things to buy, and their construction is beyond those for whom these papers are intended. Should, however, the reader fortunately be able to obtain one, he may try the following simple experiments:—Let him first turn the face of his mirror towards a landscape seen through a widow in an otherwise darkened room; by then holding a white cardboard screen in front of the mirror between it and the window (the mirror being just so much inclined that the screen can be p'aced above or below its axis), he will get a

beautiful little image of the distant landscape on the cardboard, when it is held in the principal focus of the mirror. Measuring accurately the distance between the screen and the centre of the mirror under these circumstances, he will obtain the focal length of the latter. If now at night he will place a small flame in its focal point, he will find a bright circle of light cast on the opposite wall of the exact diameter of the mirror itself; showing obviously that the rays on leaving its surface are parallel. Here, again, is an illustration that rays of light go forth and return by precisely the same route. In the case of the landscape, the parallel rays from it met in the focus of the mirror; in that of the light in such focus, the rays diverging from it were rendered parallel by their reflection from it. The reason of this the student should have no difficulty in seeing if he will only construct the diagram we have recommended him to make above. Now let him look at himself in the mirror, taking care that his face is within its focus. Fig. 11 may suffice to illustrate what will happen.

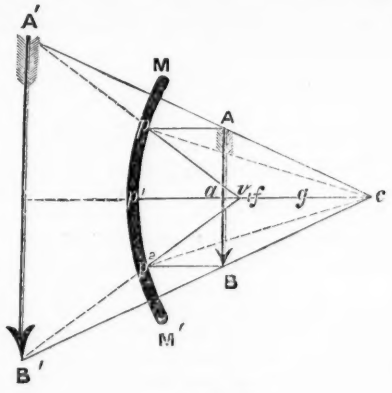


Fig. 11.

If for the reader's face we may be permitted to substitute the arrow A B, placed between the mirror M M′ and its principal focus *f*, then (remembering that, as before, radii of curvature *c p*, *c p*¹, *c p*², are perpendicular to the surface of the mirror), evidently the ray A *p* from the top of the arrow will be reflected in the direction *p v*, *a p*¹ from the middle of the arrow will return on itself, and B *p*², will be reflected in the direction *p*² *v*, and will reach the eye (suppose at *a*), as though they proceeded from the very much larger arrow A′ B′ behind the mirror. Hence an observer regarding himself in a concave mirror with his eye within its principal focus, will see a considerably magnified erect (virtual) image of himself. If we further suppose an object to be placed at some point between the focus and the centre of curvature—say at *g* in Fig. 11—a *real* magnified inverted image of it will be formed somewhere on the other side of the centre of curvature, and may be received there on a screen. The truth and reason of this, the student who has attentively followed us thus far and has taken our advice just given about drawing his own diagrams may be fairly expected to now see for himself. Should he possess a concave mirror, he should carefully repeat every experiment of which he has graphically worked out the theory. Finally, it must be evident that if we place an object at more than twice the focal length of the mirror from it, an inverted image of it, real and smaller than the original, will be formed between the focus and the centre of curvature, which is only another way of saying that image and object in our immediately preceding experiment may be made to change places. The images produced by convex mirrors, can, pretty obviously, no more be real than those seen in plane mirrors. Rays falling on them are rendered divergent, or appear to emanate from a much smaller image behind the mirror. These mirrors were at one time common articles of furniture. They are now only seen in old-fashioned homes, and rarely on those wheel-barometers, by the aid of which certain credulous people still fondly believe they can foretell the weather. They (we mean the mirrors, although it is equal applicable to the barometers) are of no practical use. The subject of reflection from cylindrical surfaces would more properly pertain to that branch of our subject dealing with distorted images; but before proceeding to examine into some of the phenomena of refraction, or the bending of the rays of light, we may refer to a simple but very effective experiment, showing the effect of multiplied reflections from the interior of a cylinder.

Get a piece of glass tube, T, rather less than an inch in diameter, and about a foot long; cover the outside of it with a piece of the dead-black paper which we have previously advised the student to obtain, the black side next the glass, and round this paste two or three thicknesses of brown paper, so as to prevent all light penetrating it. Fit a pill-box over one end of the tube, having previously burnt a circular hole, *h*, in the bottom of it with a large

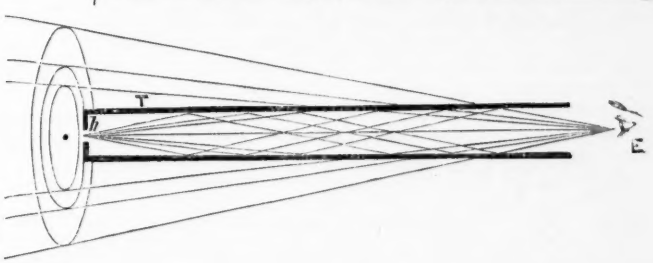


Fig. 12.

knitting-needle, made red hot. This burns the ragged edges of the hole and leaves it smooth and circular. If we now direct the hole towards a bright light, and look down the open end of the hole, we shall see a series of concentric circles of light. The course of the rays as they are reflected from the sides of the tube, until they all meet at the eye, E, is shown in our figure; a very little attention to which will enable the student, who has so far followed us, to see easily how the charming effect he will perceive in making the actual experiment is produced.

(To be continued.)

ELECTRICAL EDUCATION.

By W. Slingo.

THE science of electricity has now developed to such an extent that its applications are sufficiently numerous and important to afford lucrative employment to a host of men of all kinds and conditions. If, however, there is any profession that demands a sound scientific education it is that of an electrician. Time was, when to be acquainted with "an apparently incoherent series of facts" enabled the aspirant to claim the title. That time, however, is fast passing away, and the man who now desires to adopt any electrical application as the source of his income must acquaint himself more or less thoroughly with such laws as have so far been found to govern the science. It is not enough to know that a magnet is able to raise a piece of

iron, or that friction results in the production of electricity. He must now know how much iron, and be able to measure the electricity. Impressed with these facts, we some time since found occasion in an article on "Electrical Engineering" (KNOWLEDGE, No. 47) to speak somewhat fully of the appliances in use at the School of Telegraphy and Electrical Engineering, Hanover-square. It was there pointed out that the managers of the school were thoroughly awake to the requirements of the time, and had provided themselves with large premises, well stocked with apparatus "up to date." Since that article was penned, many important additions to the stock have been made, more especially in the electric lighting and telephone branches. Further developments are also intended, such as the introduction of mechanical engineering, so far as an electrician's requirements necessitate. For that purpose a room, light and airy, has been set apart. It may here be interesting to quote what was said on this point in No. 47:—"If there is any fault to find with the school itself, it is that it is too electrical and not sufficiently mechanical. We should much have liked to see these embryo engineers doing a little real engineering, making, repairing, and improving all kinds of apparatus, from the magnetic needle upwards." It is not, however, so much to the building and its contents that we would now direct attention as to the "education" itself, that is to say, the system of teaching and its results. Before, we could only speak of the school. We were not sufficiently acquainted with its teaching to refer to it either approvingly or disparagingly. All we could do was to venture a proviso such as "With (or, providing there were) good teaching capacity," &c. Of that capacity we have now the fullest assurance.

Recently, Mr. H. R. Kempe, author of *the* standard work on "Electrical Testing," conducted an examination of the pupils. The questions set, together with the marks awarded and the examiner's reports, have been laid before us, and most certainly show a state of things creditable alike to the school and its pupils. It appears that the system pursued is to place every new entrant into the elementary portion of the school, where his progress is checked day by day. If he fails to satisfy the tutors in this division, he is refused admittance to the advanced section, where the only thing he could do would be to impede the progress of the others. It is, however, a noteworthy fact that only the absolute dullards or positive idlers stay long in the lower class, and it is every teacher's experience that such troublesome pupils find their way into every educational establishment. The field traversed in the elementary section is a considerable one, and embraces the fundamental principles of telegraphy, telephony, electrical measurements, &c.; while the advanced class occupies itself with cable testing, the construction and principles of dynamos, motors, lamps, secondary batteries, &c., and the more advanced systems of electrical measurement. The syllabus is thus a very comprehensive one, and upon it Mr. Kempe based his questions.

We will quote one taken at random from each paper:—Elementary:—"Two batteries A and B are joined in circuit with a tangent galvanometer and a resistance x. When the two batteries assist one another, a deflection of D° is obtained; but when the batteries oppose one another, a deflection of d° is obtained. Battery B is now removed, and the resistance x is altered first to R, so that the deflection d° is reproduced, and then to r, so that the deflection D° is reproduced. Find the value of x in terms of R and r. The two internal resistances and the resistance of the galvanometer to be neglected." Advanced:—"How would you determine the current and difference of potential between the terminals of an electric lamp worked by alternating currents, the resistance of the lamp being unknown?" Mr. Kempe, reporting on the answers of the advanced pupils, says, "I must congratulate you on the most satisfactory replies which have, with few exceptions, been given to the various questions. The result is a highly creditable one to the School, and shows that the course of instruction is very complete." Speaking later of the elementary class, the examiner says:—"I must again express my satisfaction at the general excellence of the replies to the questions, and the credit which they reflect upon the School." There was a third class of new pupils whose examination was scarcely so creditable, but no importance could be attached thereto, because of their very brief training. These extracts will, we trust, speak for themselves, and warrant the eulogies which we have felt it our duty to bestow here, and in the previous article, on the School of Telegraphy and Electrical Engineering. The School is in every sense of the word commendable, and the results accomplished as the outcome of private enterprise undoubtedly present a remarkable contrast to the feeble results attained by certain endowed institutions not very far distant.

MOVED FROM AFAR.*

(*Continued from p.* 396.)

THE SENSATIONS.

PERHAPS the transmission of *localised pains* is as purely sensory an instance of telepathy as can well be selected. We have occasionally obtained this phenomenon in the normal state; but it is in the hypnotic state that, though still rare, it is most markedly induced. Take a mesmerised "subject" who is sufficently *en rapport* with his mesmeriser; talk to him on some question which engrosses his attention; and, in the middle of your talk, suddenly pinch (for instance) the mesmeriser's right ear behind the subject's back. The sleep-waker will continue to listen and reply, but his hand will fly to his own right ear, which he will rub with manifest discomfort. Now here is a transferred impression which is as purely *sensory* as we can well obtain, which prompts to an action nearly or quite reflex, and is scarcely present in any conscious manner to the sleep-waker's beclouded intellect.

Now, according to our theory of a close parallelism between the *induced telepathy* of our experiments (on a small scale) and the *spontaneous telepathy* which nature offers on a much larger scale to our examination, we might fairly expect to find some cases where a localised pain has been transferred from one person to another at a distance, unaccompanied by any definite *idea* of the cause or source of the pain thus felt. To give force to an account of this kind, it is plainly important that the pain should be sudden, distinctly localised, and not easily referable to some mere ordinary cause. If Brown were to tell us that he got into such *rapport* with Smith at a friendly Greenwich dinner that, when Smith's head ached the next morning, Brown's ached also out of sympathy, we should hand over both headaches alike to a branch of science better established than our own. But when Louis Blanc feels a shock through one of his arms, as if it had been pierced through with a rapier, at the moment that Charles Blanc's arm is pierced in a duel,† we feel that any ordinary sort of common cause for the two events is excluded. The incident which

* From a paper on Apparitions, in the *Nineteenth Century*, by Messrs. Edm. Gurney and Fred. W. H. Myers.

† See "Memoirs of C. M. Young" (1871), pp. 341, 342.

we shall now quote (occurring to Mr. Arthur Severn, the distinguished landscape-painter, and his wife, and the account of which has been obtained for us through the kindness of Professor Ruskin) presents the requisite characteristics of suddenness, localisation, and unusualness of the pain in a very high degree.

"Brantwood, Coniston, October 27th, 1883.

"I woke up with a start, feeling I had had a hard blow on my mouth, and with a distinct sense that I had been cut, and was bleeding under my upper lip, and seized my pocket-handkerchief, and held it (in a little pushed lump) to the part, as I sat up in bed, and after a few seconds, when I removed it, I was astonished not to see any blood, and only then realised it was impossible anything could have struck me there, as I lay fast asleep in bed, and so I thought it was only a dream!—but I looked at my watch, and saw it was 7, and finding Arthur (my husband) was not in the room, I concluded (rightly) that he must have gone out on the lake for an early sail, as it was so fine.

"I then fell asleep. At breakfast (half-past 9), Arthur came in rather late, and I noticed he rather purposely sat farther away from me than usual, and every now and then put his pocket-handkerchief furtively up to his lip, in the very way I had done. I said, 'Arthur, why are you doing that?' and added a little anxiously, 'I know you've hurt yourself; but I'll tell you why afterwards.' He said, 'Well, when I was sailing, a sudden squall came, throwing the tiller suddenly round, and it struck me a bad blow in the mouth, under the upper lip, and it has been bleeding a good deal and won't stop.' I then said, 'Have you any idea what o'clock it was when it happened?' and he answered, 'It must have been about 7.'

"I then told him what had happened to *me*, much to *his* surprise, and all who were with us at breakfast.

"It happened here about three years ago at Brantwood. —JOAN R. SEVERN."

"Brantwood, Coniston: November 15th, 1883.

"Early one summer's morning, I got up intending to go and sail on the lake. Whether my wife heard me going out of the room I don't know; she probably did, and in a half-dreamy state knew where I was going.

"I was left becalmed for half an hour or so, when, on looking up to the head of the lake, I saw a dark blue line on the water. At first I couldn't make it out, but soon saw that it must be small waves caused by a strong wind coming. I got my boat as ready as I could, in the short time, to receive this gust, but somehow or other she was taken aback, and seemed to spin round when the wind struck her, and in getting out of the way of the boom I got my head in the way of the tiller, which also swung round and gave me a nasty blow in the mouth, cutting my lip rather badly, and having become loose in the rudder it came out and went over-board. With my mouth bleeding, the mainsheet more or less round my neck, and the tiller gone, and the boat in confusion, I could not help smiling to think how suddenly I had been humbled almost to a wreck, just when I thought I was going to be so clever! However, I soon managed to get my tiller, and, with plenty of wind, tacked back to Brantwood, and, making my boat snug in the harbour, walked up to the house, anxious, of course, to hide as much as possible what had happened to my mouth, and, getting another handkerchief, walked into the breakfast-room, and managed to say something about having been out early. In an instant my wife said, 'You don't mean to say you have hurt your mouth?' or words that effect. I then explained what had happened, and was surprised to see some extra interest on her face, and still more surprised when she told me she had started out of her sleep thinking she had received a blow in the mouth; and that it was a few minutes past seven o'clock, and wondered if my accident had happened at the same time; but as I had no watch with me I couldn't tell, though, on comparing notes, it certainly looked as if it had been about the same time.—ARTHUR SEVERN."

It is fortunate that in this case the incident was bizarre enough to stamp itself at once on the memory. For one main difficulty in collecting cases of this sort is that, even if they do occur, they are not likely to be observed or remembered. Their theoretical importance is (very naturally) not discerned; they are thought trivial and purposeless—merely incredible, without either pathos or dignity. In reality, no narratives are more significant, or cast a more searching ray on the obscure pervasive co-sentiency of man and man.

(*To be continued.*)

Editorial Gossip.

A MR. T. W. PIPER, presumably related to the famous Peter, and inheriting some of the effects of undue pepper-picking, picks occasion to tell me, somewhat pointedly, that every reader of KNOWLEDGE is sick and tired of my "eternal quarrels" (of which I had not myself heard), and that no one will continue to take in KNOWLEDGE, if the personal peculiarities of Mr. Proctor are thus pertinaciously paraded. I have no personal objection one way or the other. If Mr. Piper is right, and he implies that he knows hundreds of readers of KNOWLEDGE all without a single exception prepared to pick the same quarrel with its editor, a score or so of others will feel the effect of their secession before (and rather more than) I shall. But that is not my point at present. I have liked to chat with readers as with friends, and Mr. T. W. Piper is one of but about six or seven who have somewhat rudely objected to this departure from customary editorial solemnity. But just now the trouble is that nearly two-score of readers have written to ask me whether certain statements about myself, which have recently appeared in the papers, are correct,—and I am divided in my mind between the objurgations of the peppery Piper on the one hand, and the suave requests of many readers on the other. But for Mr. Piper I should at once, *more meo*, have told "the truth, the whole truth, and nothing but the truth:" but seeing he knows that *every* reader would object to that process, I will simply deny categorically the untrue.

I LEARN then, to my astonishment, that I am about to settle on my wife's property in Missouri. I learn also, and again the news takes me by surprise, the following particulars of my meeting with that lady. Her name it appears was Mrs. Sadie Crowley, her husband had died in Ireland, and "having fallen in love with Mrs. Crowley on her return journey to America from Ireland," I followed her to Missouri, and in the touching language permissible at half-a-crown per thirty lines, "there wooed and won the fair widow." This is to me as full of interest as a new chapter in Reade's last novel, and for the same reason, it is so completely new to me. Though it may seem sad, I never knew a lady who either in baptismal register or among her friends went by the sweet name of Sadie. Nor have I yet had the pleasure of meeting a Mrs. Crowley, though ladies of that name are I doubt not tolerably numerous. I do not think I ever consciously knew any lady whose husband had died (recently or otherwise) in

Ireland. I did not (it so chances) meet my wife within ten thousand miles of Ireland; and I never had occasion to follow any lady to Missouri, for the pleasing purpose indicated in the papers. Nor am I to the best of my knowledge and belief about to settle on my wife's property in Missouri.

I LEARN lastly and can contradict the statement emphatically, that I have expressed an intention of ending my days in Missouri. I have no present idea of ending my days anywhere in particular. All I can just now announce as settled, in that matter, is that, presumably, I shall end my days somewhere. I hope Mr. Piper will bear with me for obtruding this information upon a perhaps impatient public.

HERE is full copy of a letter (*pace* Piper & Co.) which I have just received :—

West Hartford, Conn., May, 1884.

Richard A. Proctor.

Dear Sir,—Having heard of you through friends, I write to ascertain if you still cast horoscope's (*sic*), and what are your prices for brief delineations, also whole life reading.

Yours respectively (*sic*)

MRS. W. B. OLMSTED.

P.S. Won't you please send me a catalogue or a few testimonials?

THAT "still" goes to my heart. Do you *still* cast horoscopes? Have you left off beating your grandmother? Who can answer "yes" or "no" to such questions?

TECHNICAL EDUCATION.—The report of the Royal Commission on Technical Education, which has just been issued, goes thoroughly into the results of the investigations made by the Commissioners, who assert that England is not so far in the rear of other countries in this matter as many persons have supposed, some of our institutions, having the object of an extension of practical teaching and scientific knowledge, exciting the wonder and admiration of nations where technical schools have long been established.

AMATEUR PHOTOGRAPHY.—Although the art of photography may be said to have been in existence for five-and-forty years (since M. Daguerre and Mr. Fox Talbot both introduced their processes in 1839), but little has been done till recently to popularise what may be regarded as one of the most useful, yet simple, of all the discoveries of science. When one considers the manifold ways in which photography is a necessary adjunct to successful study, it is matter for surprise that the endeavour to introduce that art for the amateur has been so long neglected. And not only to the student, indeed, but to the pleasure-seeker is photography an accomplishment that will well repay the expenditure of time necessary to its acquirement. For instance—now that "leafy June" is here, and the days of pleasure-touring have commenced,—what is there more enjoyable than to be enabled to produce an actual presentment of some enchanting picture painted by Nature herself—"the valleys, hills, and woods in rich array"? How often does one feel the desire to recount the beauties of a favourite scene, yet with the consciousness of the utter inadequacy of verbal description? Here photography steps in. The tourist can by its means easily and quickly preserve for after-contemplation his choicest impressions, and without adding appreciably to his travelling *impedimenta*. For many other reasons is the popularisation of the art of photography a consummation to be desired; and therefore we are glad to notice that much is being done in this direction by the London Stereoscopic Company, who are prepared, not only to supply a complete and remarkably compact set of apparatus for amateurs at a comparatively trifling cost, but also to give, at their premises, 110, Regent-street, W., gratuitous lessons in the art of photography to those desirous of learning. A good feature of these lessons lies in the fact that they are not given in classes, the privacy necessary to the diffident beginner being thus ensured, and proficiency the more readily obtained. By the introduction of the "dry process," the manipulation is greatly simplified, and cleanliness in working is secured. This at once renders the art more acceptable to the non-professional student. The free-instruction system is a decidedly new and liberal departure, which should meet with the success it certainly deserves.

NOAH'S RAINBOW.

[We publish the following letter, by our ingenious and erudite correspondent Mr. E. L. Garbett, without giving any opinion on the views expressed in it, other than this, that one who admits as much doubt as Mr. Garbett does respecting the accuracy of the account of the Flood in Genesis, might well feel free to admit other and wider doubts, even such ideas as Sir George Airy's, that "we cannot doubt the Flood to have been an overflow of the Nile." The Egyptian origin of the story, touched on by Mr. Garbett at the close of his letter, corresponds with my own suggestion that the original story of the Flood was an interpretation of the pictures on some ancient constellation sphere. Just as the whole story of the Fall is found fully recorded in ancient Persian star pictures, so the story of the Flood corresponds with the course of Osiris (the sun) through the watery constellations Capricornus, Aquarius, and Pisces, to conjunction with the ship Argo (the Ark), and the long sea-line indicated by Hydra, and so past Chiron the Centaur, (identified of old with Noah), represented as offering sacrifice on the altar (Ara),—

Ara ferens thuris, stellis imitantibus, ignem,

while above, in the smoke of incense was seen the Bow of Sagittarius. All these figures were seen in the stellar heavens (in fact, few of the constellation figurings are more easily recognised among the stars than the streams from the water-cans of Aquarius, the shape of the Poop of Argo, the long sea-snake (with the Raven on its back), the Altar and the Bow; and it is fully in accordance with what has repeatedly happened in such cases, that pictures associated with these imagined figures should afterwards have a story attached to them, and that this story should be very widely spread. It is at least remarkable that the story of the Flood corresponds closely, as to time intervals with the extension of the constellations named beside the course of the sun. It is also noteworthy that the Egyptian account, which is undoubtedly the oldest (older even than the Assyrian) is associated with what is unquestionably a solar myth.—R. P.]

FEW, I suppose, have ever doubted that the bow Noah considered set in the cloud for a covenant was a natural rainbow. Moreover, nothing in the story necessitates us to suppose either he or his young disciples were not already familiar with the sight. Nevertheless, I for one have always thought it likely that they, or even their aged chief, now beheld it for the first time; which is a widely different idea from that deduced and taught, I have heard, by some mediæval divines, of its being the *first rainbow;* though you will perceive, I hope, that the moral, spiritual, or historical effect would be exactly the same. If, as I suppose, none of the eight human spectators (or none except the eldest) had seen such a thing before, the impressiveness would be just as great and memorable as if the thing were then first created.

Now there are large regions wherein natives, if not travelling beyond them, have practically no chance of seeing a rainbow, even in the longest life. To say nothing of the rainless ones (not all deserts, but comprising the fertile thousand miles of the middle Nile) all tropical islands, and perhaps most parts of the tropical continents, receive all their rain in the midday hours or by night. No rainbow is possible with a sun higher than 42°, or noticeable as a phenomenon when he is higher than 30°. To produce any in equatorial latitudes then, there must be rain suddenly leaving a spot, within the two hours after sunrise or before sunset. I doubt if this ever happens within the range of the trade-winds. Accordingly, I never met with a Jamaica negro who had seen a rainbow, or had more conception of it than of snow.

Now the civilized art of those nearest in time and place to Noah, the Assyrians, gives this decisive mark of coming from an ancestry much nearer the equator. They never represent the horned moon otherwise than quite on her back, the horns on one horizontal line. This is how no northern artists place the moon; for in fact it is not possible five degrees outside the tropics (nor except by a rare chance outside them at all) for the moon to be so seen; whereas it is her regular aspect at all hours to everyone near the equator.

Next observe that we are absolutely driven by the diluvial monuments, drift-gravel, hundred-fathom land-relics, &c., to place Noah's youth, and his progenitors, if any, in *our* "glacial age,"—in times when, as Professor Dawkins infers from the dredgings, the general sea-level was full 100 fathoms lower than now, and the lands we now inhabit were bleak icy uplands, where a thinly scattered flint-folk, like modern Esquimo, hunted the reindeer, bear, mammoth, and woolly rhinoceros. This nowise implies absence elsewhere of temperate, rich, or even hot regions, or of the highest civilizations therein. Letting alone the tropics, there were all our present shallow sea-beds, then veritable plains; the English and Irish channels, and the whole space between us and Denmark, to a north coast running from Jutland to the Orkneys. But of the Mediterranean, not only the parts thus shallow (including the

whole Adriatic and Ægean) but, as Dawkins observes, vast lower slopes, down and yet down, to even 2,000 feet lower than those, were dry. The Gibraltar lip was not yet overflowed. The ocean was not high enough to enter there, nor yet to enter the Red Sea, nor the Persian Gulf. Those were valleys below its level, but each draining into its own evaporating lake, as the little Jordan valley, or those of the Caspian and Aral do now. Of course the Black Sea was another such vale, that of Marmora another, and the two depths of the east and west Mediterranean two greater ones. In Gen. i. we read, "the gatherings of the waters called He seas," a plural phrase hardly applicable to the present world, wherein the Caspian and Aral are the biggest apart from the outer ocean; but which perfectly applied when the gatherings of all those several basins, each except that of Marmora, was a sea comparable to the Caspian. All these were at various levels (below that ocean which was 600 feet lower than at present) and thus the shores of most were lower than, and probably nearly as warm as, the hot palm-groves of Jericho. Though all present Europe were glacial then, there were for civilization these vast lowlands, besides all the tropical regions, low and high.

Now the drift phenomena everywhere compel us to see that whatever event ended that "glacial age," and introduced the present, it was but about fifty centuries ago, and yet was something prodigiously violent and sudden; more so, indeed, than divines used to gather from Noah's story, or, perhaps, that any reader would gather from that very quiet prose narrative alone. The lately-found Assyrian tablet, as interpreted by G. Smith, where the destroying goddess comes thundering with sudden darkness, and the other deities, planets, have to take refuge beyond Saturn; the Hindoo accounts that make the patriarch and his seven companions in the vessel stand aghast in utter darkness a whole night of Brahma; the Zend story that the downpour was such as to cover even hillsides to the height of a man; even the Talmudic, that says single drops were big enough to slay animals, and boiling hot,—all these hints supplementing but not clashing with Genesis, seem to me to assist us to a better conception of what could explain the visible drift geology. So, too, may the single remark of Christ, that "the day that Noë entered the ark," [not the month, but the very day] "the flood came and took them all away."

Consider the gravel-bed whereon most of London stands (nearly all its buildings prior to this reign, but comparatively little of the new suburbs.) The stones hereof are by no means pebbles, as the eocene ones of Blackheath, or those of any sea-beach. Most river stones, even of the quiet Thames, are more rounded, and all that the sea has rolled but for a few days. These have hollows untouched, edges merely blunted, are all called by some the "sub-angular drift." Very few days' rolling can they have had; and yet have all come at least the thirty or forty miles from the Chilterns, and many from the midland oolite hills, twice as far. Even those, much softer than the flints, and travelled twice as far, were not fully pebbled. What could bring stones in such quantity, so far, in so little time? The question applies equally to all valley gravels remote from glaciers, or where glaciers have plainly not been. They were once probably moraine, but since that the agent that swept them to where they are, and in a few days, must have been tremendous.

Every vale, however, on the globe that is not in new volcanic hills, testifies by its sculpture, if not by gravels like these, that its stream was once swollen, for a little time, not long, to many—shall we say hundreds, thousands,—no, to *many myriads* of times its utmost modern volume. This has happened once to the Thames, once to the Wandle, Brent, Colne, and each of its other tributaries. Was it at different times to each, I wonder? or simultaneously to them all? And if simultaneously, was there another date for the Trent and all its tributaries, and another for the Rhine system, another for that of the Ganges, and another for the Mississippi? Something may be learnt from what Lyell records of borings in the deltas of the two latter. The depth of impalpable alluvial silt, since their highest gravelly layer, compared with the regular centurial rate of each river's deposit, shows this process to have gone on about fifty centuries. It is the same with every delta examined, even down to the miniature ones of brooks entering lakes. At that particular date, every stream's present habit began. Then, and never since, did each great continental river roll down coarse gravel, hundreds of miles or leagues further than it can ever now convey more than the finest silt.

This event, the last world-wide one, cannot be described as less than the fall of a fresh sea from the sky—a single storm wherein enough water fell to raise the general ocean-level some 600 feet; to bring it up to our present coast-lines, from that old coast-line where, Prof. Dawkins says, all land relics of the glacial ages are dredged. The nearer sea-beds, all between our coast and that former one, were the lowlands inhabited by the cultured or rich contemporaries of our poor "flint folk," but retain no relics of them, because "the day that Noë entered the ark, the flood came," and swept them all away! Still more did it of course clear and scour down the lower lands around the inland seas, which the ocean, raised enough to pour through the Gibraltar Strait, now drowned and made the Mediterranean. It still has to inflow briskly, we know, to supply the evaporation; and there may be grounds to doubt whether to this day the Mediterranean be quite full, by some few inches.

The tremendous biological effects have been by nobody more appreciated, perhaps, than by the late C. Darwin, especially (but not only) while young, before becoming Selection Darwin, or Ascidian, or whatever name his later extravagances may earn. Of the extinct South American mammals (including the identical horse that Europeans have re-introduced!) he said in his "Voyage of the *Beagle*," they "lived at a late period, and were the contemporaries of the existing sea-shells. Since they lived, no very great change in the form of the land can have taken place. What, then, has exterminated so many species and whole genera? The mind at first is irresistibly hurried into the belief of some great catastrophe; but thus to destroy animals, both large and small, in Southern Patagonia, in Brazil, on the cordillera of Peru, in North America, up to Behring Straits, we must shake the entire framework of the globe." (By no means necessary.) "It appears from the character of the fossils in Europe, Asia, Australia, and in North and South America, that those conditions which favour the life of the *larger* quadrupeds were lately co-extensive with the world: what those conditions were, no one has yet even conjectured. It could hardly have been a change of temperature which, at about the same time, destroyed the inhabitants of tropical, temperate, and arctic latitudes, on both sides of the globe. In North America, we positively know from Mr. Lyell, that the large quadrupeds lived subsequently to that period when boulders were brought into latitudes at which icebergs now never arrive. From conclusive but indirect reasons, we may feel sure that, in the southern hemisphere, the *Macrauchenia* also lived long subsequently to the ice-transported boulder period. Did man, after his first inroad into South America destroy, as has been suggested, the unwieldy *Megatherium* and the other Edentata? We must at least look to some other cause for the extinction of the little *Tucutuco*, and of the numerous fossil *mice*, and other small quadrupeds. No one will imagine that a drought, even far severer than those which cause such losses in the provinces of La Plata, could destroy every individual of every species, from Southern Patagonia to Behring Straits. What shall we say of the extinction of the *Horse?* Did those plains fail of pasture, which have since been overrun by thousands and hundreds of thousands of the descendants of the stock introduced by the Spaniards? Have the subsequently introduced species consumed the food of the great antecedent races? Can we believe the *Capybara* has taken the food of the *Toxodon*, the *Gnanaco* that of the *Macrauchenia*, the existing small Edentates of their numerous gigantic prototypes? Certainly no fact in the long history of the world is so startling as the wide and repeated exterminations of its inhabitants."—"Journal of Researches," 1845, c. viii.

Equally insoluble he found afterwards, in the famous book so oddly misnamed "Origin of Species," at least two world-wide facts of the present life-distribution. Our planet has, for organisms, three mansions—land, sea, and fresh water. Now the sea is practically all connected; and so is a majority of the land in one mass, or even its whole main bulk, all but the islands, in three masses. But the fresh waters are in thousands of disconnected basins, and the very largest hardly perhaps a tithe of the whole. Creatures that cannot leave fresh water can no more reach the Thames from the Trent than reach another planet. "We might expect then," said Darwin, "that the existence of ubiquitous tribes, or even very wide-spread ones, would be common only in the ocean, or the air-breathers of the dry land; and that the fresh-waters would be characterized by the absence of these, and prevalence of localized species peculiar to their own small districts. But the case is exactly the reverse. Not only have many fresh-water species an enormous range, but allied species prevail in a remarkable manner throughout the globe. When first collecting in the fresh waters of Brazil, I well remember feeling much surprize at the similarity of the freshwater insects, shells, &c., and at the dissimilarity of the surrounding terrestrial beings, compared with those of Britain." He takes all the rest of the chapter in illustrating the extreme difficulty of accounting for any migration, even the smallest, of freshwater beings from one river to another,—the elaborate ingenuity he needed to make it at all possible; which of course is not even touching his problem, why the freshwater mansion should be the *most* uniformly stocked, instead of the *least* so! According to Genesis, however, as the diluvial waters fell from heaven, they were fresh; and so all the fresh waters of the globe

XUM

have been in temporary connexion *more recently* than the lands, or even seas of different climates.

His other grand puzzle was about oceanic islands; but first it seems that, before immigrants brought mammals, no such island contained any, except in some cases bats. These, he remarks, could fly thither, but other mammals could not naturally be conveyed from the continents. Now, as he insists that many islands are as ancient as the continents, we must ask, on his principle of "*evolution*," why has there not been time for as many or as fit species to be developed on each of those islands as on the continent itself? Why talk of the difficulty of any getting *to* the island *from* the continent? Why more need of migration to island than to continent?

But the grand fact was that, in each island, the few species native there, and even the still fewer, in some cases, peculiar thereto, are so far from being the fittest to it that the world contains, that these insular species, animal or plant, are often quickly exterminated by the competition of some mere come-by-chance from a continent. "He who admits the doctrine of the creation of each separate species, will have to admit that a sufficient number of the best adapted plants and animals were not created for oceanic islands; for man has unintentionally stocked them far more fully and perfectly than did nature." What is this but denying that his principle of natural selection has operated on islands at all? But why has it not? No shadow of a reason appears. But Genesis explains perfectly. The few species on each island from Noah's time till it is colonized by ships, have no fitness, because mere accident threw them there. They are simply those, fit or unfit, that, in the rush of the deluge, happened to be stranded thereon. It is the rule for most of these ultimately to be superseded by some that our ships introduce; each island thus repeating in small the marvel of those American horse-pastures, where the fossils show that the Horse swarmed with the Mastodon and Megatherium, as it swarms again now, but between the ages of Noah and Columbus was unknown.

I submit that nothing has the least explained the drift and valley phenomena, but the old ubiquitous tradition that, on a certain day, a general cataract descended. It was a work of but minutes for all timber, vegetation, and human structures to be uprooted and afloat, with all the matted forests of the tropics, all glaciers, and the ice of both poles. Now wherever such a vessel as Noah's might be built, the effect would plainly be to hurry it, at railway speed, along the directest waterways to the ocean. In whatever continent it was, if at all central, its course and issue to sea would be by that continent's present chief river mouth, or one of the chief. And once at sea, we can hardly conceive a chance of its ever grounding, unless on some newly upheaved volcanic shoal, providentially stopping it when its momentum was just about spent. Can we find any of the few great continental streams with a rather straight issue to sea, and in the direction of which issue, continued some few hundreds of miles, recent volcanic land appears? Only one great river and one post-tertiary volcano can be found, I believe, thus related; and the situation of the latter is most unique, in the very heart of the old world, overtopping all mountain chains within 1,000 miles, and almost every point of them, yet not itself belonging to any, and the centre of such a circle as I proposed the enigma of finding, but which none of your readers has attempted, an unequalled circular area, chiefly of land, yet which no fresh water flows *from*, but many rivers from all the three continents enter it, and terminate in inland seas within it, seas that, though inland, mostly have connection with the remote ocean.

There is no upheaved mass but historical ones, like the Jorullo of last century, that geologists venture to pronounce newer than Ararat. It had an eruption as lately as 1840, destroying many lives, a monastery and a village, traditionally the site of Noah's first vineyard. In Johnston's Atlas, it is coloured as the very largest patch of the newest sort of volcanic land. Now the chief of those rivers running so strangely towards it, is the longest of Africa and most famous of history. We are almost compelled to infer that, if Noah's story have any foundation, his original country was on the upper Nile. From any of its upper reaches or tributaries anything movable would, by the "palœozoic flood" (as the drift catastrophe has lately had to be named) be swept in very few days along that stream, out of its old eastern mouth, over Pelusium, and so north-eastward, with gradually slackened speed, till gently stopped by whatever part of northern Syria or Armenia first emerged. "*Emerged*" is a word I think applicable to that region, but to no other; because the tremendous downpour (averaging some 400 feet universally) though not causing disturbance of the earth's crust elsewhere, was most likely to do so around the newly filled basins of the inner Mediterranean, the additional weight now accumulated on them, the Euxine and Levant, as also in the enclosed Caspian basin, being (according to Dawkins) five or six-fold what was added to the oceans generally. This would be likely to produce in Western Asia, if *nowhere else*, depression and submergence of those countries, which I suppose to have been some months rising again.

Away from that region, here and generally, everything indicates that "all the high hills under the whole heaven were covered," but not, as vulgarly fancied, with *standing*, but with *running* water; which in a few days scoured their rocks bare, swept off to sea all movables lighter than boulders (many of which were carried miles) scooped new valleys, and filled the hundreds of bone-caves. But Buckland and Cuvier certainly erred in calling it "the birthday of our continents." It was only a baptism, and not generally one of immersion; only that, I hold, for the region round Ararat. There, as Noah recorded, when grounded, on Easter Day, the 150th he had been afloat, "all the high hills that were under the whole heaven" remained still covered *from him*, as they are now from any ship in mid-ocean. Meanwhile plenty of remote lands, perhaps the majority of those beyond his ken, were reclothing themselves with whatever seedlings the vast flotillas of matted forest happened to bring them; and many a small animal may have clung thereto and sustained life, though "all in whose nostrils was the breath of life, of all *in the dry land* died."

There are many caves however, the habitations either of day birds by night, or of bats and night birds by day, and any such cave whose entrance has no descent, might, I suppose, preserve some of its inmates if they found food. The reclothing of some lands we know proceeded so (under what must have been the hottest summer they had known for ages) that, on the 198th day from the catastrophe, a day afterwards to be known as Pentecost, an olive-leaf was brought to Noah by one of his birds. Why Jews now keep their Feast of Weeks the 51st day from their Passover, when the Bible seems to make it the 50th, I know not. But certainly the memorable Pentecost of Acts ii. cannot have been the 52nd, like our Whitsunday. If it was, as Exodus seems to require, the previous Friday, that would correspond to Noah's dove's return.

But when the surrounding lowlands, with wreckage of a drowned world, were to emerge under that broiling summer, it was essential to our ancestors to have purer atmosphere. They were kept imprisoned then, waiting the order to disembark, while the shoal that had stopped them, at our present sea-level, by repeated little shocks and heaves ("the waters were going and returning" says the Hebrew) gradually hoisted them above the clouds. The whole upheaval was little faster than that of the new Aleutian isle in this century, and no single hoist may have approached that of the Monte Nuovo in one night, far less that of Jorullo. Of course such a mountain may have changed considerably since, but it is plain that Noah never knew of a higher than it was when he descended. He could not otherwise have held that all hills had been covered. He probably believed they had been with *standing* water, and so may all have been that he afterwards knew. And all on the globe, including his native ones, had been so with *running* water.

Now if he came from the upper Nile, whether Abyssinia or the rainless parts, it was from lands of no rainbow practically. To his family, if not himself, that seen on Ararat would be the first. But there is much traditional ground for holding the above was his voyage. Ham, afterwards worshipped as Jupiter Ammon, was held to have founded Thebes, whose name is the very one Genesis gives the ark; and down to Plutarch's time divine honours were paid there to a gilded ship 250 cubits long. Was not Ham's the very family to begin such worship, at the place where he either knew or believed that he had worked at his leader's ark? But further, their legend of Osiris, annually celebrated, was that the corpse of that symbolic person, representing humanity destroyed and revived, was launched in a boat down the Nile, when the sun is in Scorpio and the seventeenth day of the moon (the very month and day of the deluge), and being carried out of its eastern mouth, grounded on Northern Syria, there to be cut in pieces that repeopled the world.

E. L. GARBETT.

INTERNATIONAL HEALTH EXHIBITION.—We are reminded that this is not the first Health Exhibition held in London, for one was opened last June, under the auspices of the National Health Society, at Humphrey's Hall, Knightsbridge. That Exhibition, though small, was a great success, and no doubt suggested the idea of the larger and more comprehensive one now at South Kensington. The National Health Society is represented at Kensington by a modest stall in the Eastern Quadrant, near the Conservatory, where cheap books and leaflets of a popular description are displayed. They relate to ventilation, house sanitation, prevention of the spread of fever, the management of infants, food, cookery, &c. A conference on school hygiene will be held at the Exhibition in August by the National Health Society. The meetings in relation to this conference are being held at the society's rooms, 44, Berners-street, W.

"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the EDITOR OF KNOWLEDGE; *all Business communications to the* PUBLISHERS, *at the Office, 74, Great Queen-street, W.C.* IF THIS IS NOT ATTENDED TO DELAYS ARISE FOR WHICH THE EDITOR IS NOT RESPONSIBLE.

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THE RECENT EARTHQUAKE.

[1297]—The shock of the earthquake was experienced very strongly and distinctly by the occupants of one of the top flats (No. 37) of Oxford Mansion.

The water to this mansion is supplied from an artesian well, and for several days after the earthquake the water was considerably mixed with a white earth, of chalky appearance. Being quite unfit for any purpose whatever, men were sent for to find out what was wrong, but all their examinations were useless—the water was simply composed of half chalk. Suddenly it righted itself just as mysteriously as it went wrong. C. L. H. WALLACE.

CO-INCIDENCES.

[1298]—The correct answer to the question in Letter 1283 (KNOWLEDGE, No. 135), is "Viscountess Beaconsfield," of whom all the assertions contained in that letter are literally true. It is, perhaps, not necessary to observe that Mr. Disraeli was not raised to the peerage till some time after the death of his wife.—Yours, L. E. B.

[1299]—I was much struck by the curious coincidence forming the subject of letter 1283. I saw at once that the person described could not be George Eliot, as she was said to have "married" a gentleman of the name of Lewis (not Lewes); but I was completely at sea as to who else could suit the other points of the description. Doubtless many other readers of KNOWLEDGE had exactly the same experience, but mark what follows. I had carried home with me from a public library Foster's "Members of Parliament for Scotland, 1357–1882," which I had borrowed with the view of ascertaining the representatives in the seventeenth century of a certain Scotch burgh. Laying down KNOWLEDGE, I took up this book, and on opening it, my attention was at once caught by an advertisement (inserted at the beginning) of the same author's "Collectanea Genealogica," including a specimen page. This page contained the descent of the Disraeli family, and actually the first words that caught my eye were:—

"Benjamin, Earl of Beaconsfield = Mary Anne, Viscountess Beaconsfield in her own right, so created 30 Nov., 1868; widow of Wyndham Lewis, esq., M.P., of Pantwynglass Castle, co. Glam., and only dau. of Capt. John Viney Evans, R.N., of Bampford Speke, Devon, m. 28 August, 1839, d. 15 Dec., 1872, bd. at Hughenden, M.I., aged 76.

Of course, in the text of Foster's "Members of Parliament for Scotland" there is no reference whatsoever to Disraeli. P. J. A.

MAN'S BRAIN, &c.

[1299] Will you kindly permit me to ask two questions of Mr. Edward Clodd?

(1) In KNOWLEDGE for to-day (May 30) he states, under the heading "Dreams":—

"The cranial capacity of the modern Englishman surpasses that of the aboriginal non-Aryan Hindu by a difference of sixty-eight cubic inches, while between this Hindu skull and the skull of the gorilla the difference in capacity is but eleven inches."

Will Mr. Clodd kindly state his authority for this fact?

(2) He also declares it a "fond delusion" to "place man in a kingdom by himself and deny his pre-human ancestry."

Is not such "fond delusion" the deliberate judgment, based on adduced evidence, of Professors Owen, Carpenter, and de Mivart, Alfred Wallace, and J. W. Dawson?—Yours, &c.,

S. STRINGER BATES.

LETTERS RECEIVED.

S. The binocular microscope gives a pleasant form of vision with low powers; but its value rapidly diminishes with higher ones. Every notable discovery hitherto made has been with the monocular form of instrument.—W. GOWINGS, T. A. E. SANDERSON, and others. Thanks for all the trouble you have taken, but see Mr. Dodgson's letter.—A. J. W. Your reminiscences of George Eliot extend to rather too great a length, in the terribly overcrowded condition of our pages; a remark which extends to the "coincidence" of meeting your sister's former *fiancé*.—W. O. AINSWORTH. If I could only spare you half a number of KNOWLEDGE to yourself! Your "Nonsense attached to the Flat Earth Theory," would, doubtless, make Parallax tear his hair, and drive Mr. Hampden into an asylum; but——.—W. HUME suggests that "Phoro" (letter 1245), should exercise his hand; and tells a story of a cat that imitated a bird's note. (This was probably for a-mews-meant.)—ENGLISHMAN. I suppose that German gardeners work for less than our own countrymen.—O. B. Ob-viously so.—H. W. CLARKE. I know of no one who gives instruction in practical astronomy. Loomis's work on that subject is most explicit; and nothing but actual practice with the instruments described in it will make you expert in their use.—C. H. C. The *South London Press* simply stated "the thing which is not" in the farrago which you quote. "Mr. Richard Proctor" never "assigned fifteen years"—nor fifteen hundred, nor fifteen thousand years—"as the residuary term of human existence in our sphere." He has an abiding conviction that the world will last at least as long as the newspaper which you quote.—JOHN F. WILKINSON. I do not know what books are read for the B.A. Exam. of the London University, but Godfray's "Astronomy" (a Cambridge text-book) ought to enable you to answer any question in the least likely to be put.—R. H. R. If you reflect for a moment that the heavens seem to rotate round an axis inclined to the horizon, you will see at once that what is the top, or north point of the sun when he is on the meridian will appear to be on one side of him as he nears the horizon. Get a celestial globe, elevate it to the latitude of Eastbourne (about 51°), cut a circular disc out of note-paper, and stick it on to the globe. Bisect this by the brass meridian and make a pencil-mark at the top of it. Now turn the globe round its axis until your disc is some hours east or west of the meridian, and note in what direction your pencil-mark lies. Or when the moon is nearly full, hang a plumb line in front of her, and note where it cuts her limb. Do the same thing four or five hours afterwards, and observe how differently she seems to lie with reference to your line.—JAMES P. CALDERON. Less than nothing is known as to the *cause* of gravitation. No more information on the subject was given, because there was none to give.—JNO. S. HEPBURN claims to have invented a telegraph cable in which kinking is rendered impossible by swivel couplings.—MRS. C. L. H. WALLACE thinks that "very valuable vegetable victuals" ("Spell it with a 'We,' Samivel") have been spoken of too slightingly in these columns. I regret if this is so; but meanwhile shall, I am afraid, continue to flavour my asparagus with some lamb, take duckling as an occasional adjunct to peas, and relieve the monotony of spinach and potatoes with a modicum of beef and mutton.—H. A. B. I believe that it was the heliacal rising of Procyon—the "Canicula" of the Romans—which determined their dog-days. These lasted from July 3, to Aug. 11; dates still retained in some of our almanacs; albeit it is the end of August now ere Procyon rises with the sun.—THOS. FIELDING. See if you can pick up Braid's "Magic, Witchcraft, Animal Magnetism, &c." in any public library, or at a second-hand scientific bookseller's.—J. There can be no objection to the adjective you employ as describing the book of which you speak; but it is equally applicable to Plato, to Shakespeare, Newton, and others their fellows. How can you possibly prove a miracle by documentary evidence?—WILLIAM CLARK. C. F. Hodgson & Son, Gough-square, Fleet-street, publish the works on the Hamiltonian system.—J. C. MURRAY AYNSLEY. You appear to be unable to discriminate between what is absolutely opposed to irrefragably established scientific facts, and what is, so far, unexplained. Granted that the "homing" faculty of birds (by no means so infallible though as you assume) has not yet been quite unriddled; what argument does that supply for the existence of death wraiths? I don't see the sequence myself.—R. The earth is an oblate spheroid; *i.e.*, it is flattened at the poles, and more convex at the equator. Consequently the polar region must form an arc of a larger circle than the equatorial one, and

they quite obviously could not be struck from the same centre, the polar *radius* being greater (although the polar axis is the less) of the two. But then a degree is the 360th part of a circle, and the 360th part of a big circle is greater than the 360th part of a less one, Q. E. D.—H. MARFLEET. You are very kind indeed; but neither Mr. Slack nor Mr. Mattieu Williams seems to care about having your two bottles of mouldy fruit. — C. HURST. Your very plain and practical ethical conclusion that "things are only in their own nature bad in so far as they are injurious," should commend itself to popular acceptance. — REGINALD N. ROGERS. The phenomenon you describe is one of the most familiar ones possible to all navigators and residents by the seaside. I have seen it at Worthing over and over again. It is, as you say, a kind of mirage, and has its origin in the unequal heating of the strata of air immediately above the surface of the sea.—JOHN BELL. The only feasible explanation that occurs to me is that your uncanny windows were glazed with very inferior and bent glass, and that as you varied the angle at which a line from your eye met the crooked panes, part of them reflected the light of the sky and part of them did not. *Re* the so-called "muscle-reading." A modification of it has been a fashionable game in hundreds of drawing-rooms for a long time.—W. I sincerely regret that the great length of your investigation of the question put in letter 1274, compels me to exclude your pretty proof of the general rule. If you could only see the piles of communications lying before me as I write!—J. W. The picture given was remarkable for the singularly rich display of *faculæ*. In your picture there was nothing unusual; but assuredly no offence was intended, as you seem to surmise. I acted according to my knowledge of what is or is not likely to be interesting.—H. A. B. *Real* origin of the term Dog Days not known. Probably name was given at a time when sun and Sirius were in conjunction at the time of year to which the term is now applied.—DIOGENES. Am very much of your mind.—WILLIAM CLARK. Regret that space will not permit insertion of your rather too elaborate demonstration.—PRO BONO PUBLICO. There is no real resemblance between the two systems. Were not the terms of your letter purposely offensive, I would have answered at greater length.—H. H. TANKARD. See advertisement on last page of each number.—T. W. P. Eternal quarrels! My dear sir, what *are* you thinking of? I have not been engaged in anything of the nature of a quarrel for years. There are not ten men in the world, I believe, who even imagine that I entertain feelings of hostility towards them; and the few who do are mostly mistaken. Possibly if there were many who wrote as—well as *crisply*—as you do, some quarrelling might arise. And yet, I know not: it takes two to make a quarrel. To turn to the sensible part of your letter,—it has been by no means my fault if the papers whose style and quality you so justly admire have appeared lately at longer intervals than before. Matter cannot be set up before it is received, nor engravings prepared without the necessary "copy." On another point,—if I were to draw up, for you a short and true account of the sums respectively received from KNOWLEDGE by the "others" whose cause you take up, and by the "one" who has, you think, taken the lion's share (he has of the *work*, I can assure you), you would probably change your tone a little. I could point to numbers which brought him, for all his work, scarce anything at all. —A FACTORY HAND, ENFIELD. I cannot help you; I can give you a reason; but what can you do with it, having so little understanding?—T. H. I scarcely needed your long letter to know how you view the books to which you refer. I view them quite differently, and regard them as quite unworthy of the attention you wish me to give them.—H. GOLDZIHER. The question is one of nomenclature. In the words, billion, trillion, quadrillion, and so forth, we have the numbers two, three, four, and so forth: the idea conveyed is that a billion represents some number to the *second* power, a billion the same number to the *third* power, a quadrillion the same number to the *fourth* power, and so on. Accordingly our English usage gives:—

A billion $= (1{,}000{,}000)^2$
A trillion $= (1{,}000{,}000)^3$
A quadrillion $= (1{,}000{,}000)^4$

and so on. The only way of working in the numbers 2, 3, 4, &c., in connection with the American system is thus

A billion $= (1000) \times (1000)^2$
A trillion $= (1000) \times (1000)^3$
A quadrillion $= (1000) \times (1000)^4$

and so forth. Your plan which would make a billion equal to a million million, a trillion equal to a billion billion, a quadrillion equal to a trillion trillion, and so forth, seems hopelessly to lose the effect of the bi-, tri-, quadri-, &c., in the compound words; for you have

A billion $= (1{,}000{,}000)^2$
A trillion $= (1{,}000{,}000)^4$
A quadrillion $= (1{,}000{,}000)^6$

and so on.—F. M. BILLINGS. The work of which you speak as "recently published" was stereotyped in 1877! I certainly found nothing in Mr. Clissold's remarks to cause me to have the plates altered. The statements with which I dealt were taken directly from a work published by Swedenborgians, and bearing their imprint. I believe now as I believed when I wrote the article, that Swedenborg was misled by wild and visionary dreams, the offspring of a partly-diseased imagination, stirred by such astronomical facts as were known to him and such astronomical ideas as were prevalent in his day. It is a reasonable and, withal, a kindly interpretation. Mr. Clissold maintained a different opinion, on grounds which seem to him sufficient, to me otherwise. I really am not aware of any law of "courtesy" requiring me to insist on the flaws, or what seem to me such, in criticisms made on my works. As an illustration of my position, consider your remark that "Mr. Clissold proves" (*sic*) "that unless Swedenborg had suspected the existence of Neptune he could neither desire nor obtain intercourse with spirits therefrom," "and yet" you say, I "persist in" my "objection" that if knowledge were obtainable by Swedenborg's vision method he might have been expected to tell us something about undiscovered planets. Mr. Clissold's proof is no proof for me. He believes in Swedenborg's imagined power of communicating with spirits from other planets, and believes in such limitations as are absolutely essential if that belief is to have any logical stability; I neither believe that Swedenborg had any such power, nor that Swedenborg himself recognised any limits to the power he supposed he possessed. Interpretations put by Mr. Clissold on the words which Swedenborg himself wrote, are simply valueless for one who prefers to take those words as they stand. You can by suitable interpretations show that Swedenborg knew all that modern science has discovered, just as Mr. Kinns by suitable misinterpretations has done for still older writings, or as Professor Symth has done for the Great Pyramid. But if these interpretations are simply rejected—as they must be by every one who looks on them without prejudice—they count for nothing.

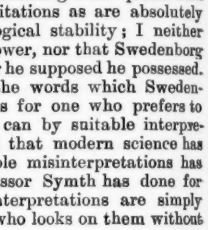
Our Mathematical Column.

NOTES ON EUCLID'S FIRST BOOK.

By RICHARD A. PROCTOR.

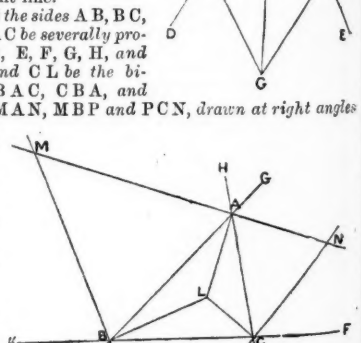
(*Continued from p.* 403.)

PROP. XXXIII.—*In the triangle* A B C, *let* B F, C F *be the bisectors of the angles* A B C, A C B; B G, C G *the bisectors of the exterior angles* D B C, B C E: *then shall the points* A, F, *and* G *lie in a right line.*



For by Prop. XXX. the point F lies on the bisector of the angle B A C, and by Prop. XXXII. the point G lies on the same bisector. Hence the points A, F, and G lie in one straight line.

PROP. XXXIV.—*Let the sides* A B, B C, C A *of the triangle* B A C *be severally produced both ways, to* D, E, F, G, H, *and* K, *and let* A L, B L, *and* C L *be the bisectors of the angles* B A C, C B A, *and* A C B: *then the lines* M A N, M B P *and* P C N, *drawn at right angles to* A L, B L, *and* C L, *shall bisect the angles* H A B *and* G A C, A B K *and* D B C, B C E *and* A C F.



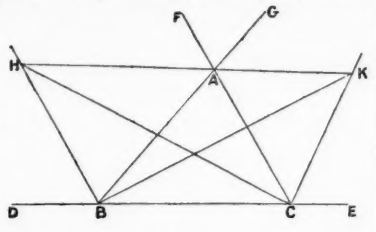
For the right angle L B M is equal to the right angle L B P, whereof the portions A B L, L B C are equal. Hence the remaining angle M B A is equal to the remaining angle P B C. But the angle M B A is equal to the vertical angle D B P, and C B P to M B K (Euc. I. 15). Hence the four angles A B M, M B K, D B P, P B C are all equal, or P M bisects both the angles A B K and C B D

Similarly P N bisects both the angles A C F and B C E; and M N bisects both the angles C A G and B A H.

COR. 1.—A line which bisects any angle bisects also (when produced) the vertical angle.

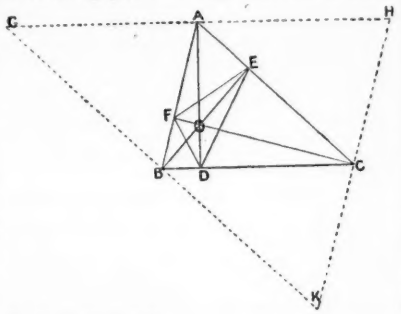
COR. 2.—The bisectors of the two pairs of vertical angles formed by two intersecting lines are at right angles to each other.

PROP. XXXV.—*Let* B C, *a side of the triangle* A B C, *be produced either way to* D *and* E, *and let* B A, C A, *be produced, respectively, to* G *and* F: *then, if* B H, C H, *the bisectors of the angles* A B D, A C B, *meet in* H, *and* C K, B K, *the bisectors of the angles* A C E, A B C *in* K, *the points* H, A, *and* K *lie in a straight line, and this line bisects the angles* F A B *and* G A C.



For, by Prop. XXXII. Cor., A H bisects the angle F A B and A K bisects the angle G A C. But the angle F A B is equal to the angle G A C. Therefore the angles H A B, G A K being the halves of these equal angles are equal. But H A B and G A K are vertical angles. Therefore H A and A K are in the same straight line, (Euc. I. 15, *Conv.*), and it has been shown that H A K bisects the angles F A B and G A C.

The *method* of the following proof of Prop. XXVIII. is worth noticing.



First, let the triangle A B C be acute angled. Draw A D perpendicular to B C and B E perpendicular to A C: then if it can be shown that C O produced cuts A B at right angles it will be obvious that Prop. XXVIII. is established, since there is only one perpendicular from C on A B.

Join D E. Then the angles of the quadrilateral O E C D are together equal to four right angles (Euc. I., 32, Cor. 1.); therefore since the angles O E C, O D C are right angles the angles D O E and D C E are together equal to two right angles. But D C E is acute (*hyp.*) therefore D O E is obtuse; and the angles O E D, O D E are, therefore together less than a right angle (Euc. 1., 32). Hence if we make the angles O E F, O D F equal to O E D, O D E respectively, the two angles D E F, E D F are together less than two right angles (being double of the two angles O E D, O D E together). Hence E F and D F meet, as shown in the figure. Join O F. Now in the triangle F E D, O E and O D are the bisectors of the angles F E D, F D E; therefore A E C and B D C are the bisectors of the angles external to F E D, F D E (Prop. XXXIV.). Hence by Prop. XXXV., F lies on the line A B and A F B is the bisector of the angles external to D F E. Also F O is the bisector of the angle E F D (Prop. XXIX.). Therefore A B is at right angles to F O (Prop. XXXIV., Cor. 2). But the points C, O, and F are in one straight line (Prop. XXXIII.). Hence C O F is at right angles to A B.

Secondly take the case of the obtuse-angled triangle A O B. Produce A O to D and draw B D perpendicular to A D. Produce B O to E and draw A E perpendicular to B E. Then the angles A E D and B D E are each greater than a right angle, so that A E and B D must meet if produced towards E and D. Let them meet in C. Then we have to show that a perpendicular from O on A B will, if produced, pass through C. Now since O E C and O D C are right angles and D O E is obtuse, the angle E C D is acute (Euc. I. 32, cor. 1); also since A E B and A D B are right angles the angles E A B and D B A are acute (Euc. I. 17). Hence in the acute-angled triangle A B C, the perpendiculars B E and A D intersect on the perpendicular from C on A B. That is the perpendicular from C on A B passes through O; or in other words, the perpendicular from O on A B, produced beyond O passes through C, the point of intersection of A E and B D produced.

SCHOLIUM.—It is worthy of notice that if we take any triangle G H K, bisect its sides in the points A B C and form the triangle A B C, and again draw the perpendiculars A D, B E, and C F, and form the triangle D F E, then the three important properties contained in Props. XXVII., XXVIII., and XXX. are illustrated together; since the same three lines A D, C F, and B E are at once the bisectors of the angles of the triangle D F E, the perpendiculars from the angles on the opposite sides of the triangle A B C, and the rectangular bisectors of the sides of the triangle G H K.

(*To be continued.*)

Our Whist Column.

BY FIVE OF CLUBS.

WONDERFUL LUCK AT WHIST.

THE following from an old number of the *Westminster Papers* will be found interesting. Mr. Proctor would probably find it rather difficult to determine the odds against the singular series of hands described:—"In the course of the last month we played Whist at the Whitehall Club. Three members of the club took part in the rubber. In the third hand it was our deal; the pack had therefore been played with once. We dealt ourselves Ace, King, Queen, Knave, ten, eight, and another trump. In due course it again became our deal, and we gave ourselves the same trumps. The third time we dealt we had Ace, King, Queen, Knave, ten, and another trump, but in a different suit. The fourth time the Quint Major and one trump; the fifth time the Quint Major; on the sixth, Quint Major, eight, and another trump; on the seventh deal our partner dealt for us. In our hand we found Ace and eight of trumps, and in our partner's, King, Queen, Knave, ten, and nine. Probably such a combination never occurred before and may never occur again. The odds against it seem so great that we commend the example to Mr. Proctor, assuring that gentleman that he can have the names of the other players for his own satisfaction. Now, this is pure luck; no skill could avail against such hands as these. This week we saw a player lose seventeen consecutive bets on seventeen even events, and we think it is because of occasional runs of luck such as these that many players look on Whist as a game of luck, and ignore the subject of skill. The same luck exhibits itself in the more scientific game of Double Dummy, only here the skill can be demonstrated to any one's satisfaction. A player may fancy that he is as good a Whist player as any one else; the fact that he loses a trick here and a trick there makes no impression, because he thinks that others make as many or more blunders, or he does not believe that he loses a trick at all. The actual and respective skill of any given players cannot, therefore, be ever demonstrated to the satisfaction of either. But take a Double Dummy player, and tell him to make the most out of a given hand, and then let his supposed equal try the same hand against the same opponent; if it turns out that the one player habitually does better with the same hand, he might acknowledge his inferiority. This could be tested by natural hands, and could be further demonstrated with the assistance of problems set by a good composer. But players say, although I play Double Dummy, I am not a problem solver. Every Double Dummy hand is a problem of more or less difficulty. It is not Whist, it is true, but the ending of a Whist hand is usually a problem. It has never been tried, to our knowledge, to have a real match, in which a sufficient number of hands have been arranged and reversed, so that each pair of players play with the same hands and at the same score. The trouble would be great, but the lesson to be taught would be of value to the card world.

ELECTRIC LIGHTING AT HULL.—At a recent meeting of the Lighting Committee of the Hull Corporation the subject of the electric lighting contract with Messrs. Siemens was gone into. The borough engineer laid before the committee the following letter from Messrs. Siemens:—"We are quite willing to let the electric lighting plant remain at Hull on hire after the expiration of the present contract (July 28, 1884), subject to the following conditions:—1. That the plant be handed back to us at the end of the period of hiring in the same condition, fair wear and tear excepted, as it is handed over by us to the corporation at the end of the present contract, unless the plant is purchased by the corporation during or at the expiration of the period of hiring. 2. That the plant is hired for at least one year from the expiration of the present contract, and that after that time either party can give three months' notice to determine the contract. 3. That the corporation pay us a sum of £476 per year in four equal instalments, payable at the end of each three months." The borough engineer expressed the opinion that the terms contained in this letter were favourable, and should be accepted. The committee having discussed the matter, resolved to endorse the borough engineer's opinion.

Our Chess Column.

By MEPHISTO.

"I URGE pupils to learn Chess, and enjoy it myself, to the point of its becoming a temptation to waste of time often very difficult to resist; and I have really serious thoughts of publishing a selection of favourite old games by Chess-players of real genius and imagination, as opposed to the stupidity called Chess-playing in modern days. Pleasant 'play,' truly! in which the opponents sit calculating and analysing for twelve hours, tire each other nearly into apoplexy or idiocy, and end in a draw or a victory by an odd pawn. "JOHN RUSKIN."

SOLUTIONS.

PROBLEM No. 119, BY H. W. SHERRARD, p. 404.

1. Kt to Q4 if
   1. K × Kt — 2. Q × P, mate
   if 1. K to Q3 — 2. Q to R 3, mate
   if 1. P to Q3 or B4 — 2. Kt to B3, mate
   if 1. B moves — 2. Q to R2, mate

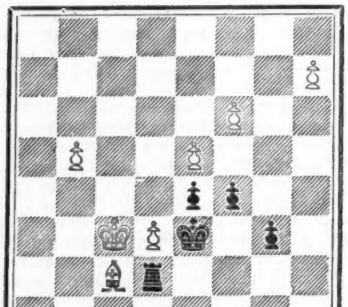
PROBLEM BY S. LOYD.

1. Q to Kt4 (L) if
   1. P to B4 — 2. P × P en pass, mate
   if 1. K to K2 — 2. B × P mate
   if 1. K to K4 or Q 4 — 2. Q to K4 mate

The following position occurred in a game played in the Divan Tourney:—

MR. HIRSH.

WHITE.



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CHESS EDITOR.

White can only draw by playing

1. P to Kt5	1. R to R2
2. P to Kt6	2. R × P
3. P to Kt7 (*a*)	3. R to B7 (ch)
4. K to Kt6	4. R to Kt7 (ch)
5. K to R6	5. R to R7 (ch)
6. B to R5	6. R to Kt7
7. B to Kt6.	

If, instead of 7. B to Kt6, White plays 7. K to R7, then K × P. 8. P to Kt8 (Q), R × Q. 9. K × R, P × P. 10. P × P, K to B4. 11. B to B6, K to B5. 12. B × P, K to K6, and draws.

	7. R to R7 (ch).
8. K to Kt5	8. R to Kt7 (ch).
9. K to B5	9. R to B7 (ch)
10. K to Kt4	10. R to Kt7 (ch)

Drawn game by perpetual check.

(*a*) This is a good position, and yields some interesting play, notably if White on his third move plays K to Kt7. In every case, however, a draw will result, with best play.

PROBLEM (SELECTED).

By G. E. CARPENTER.

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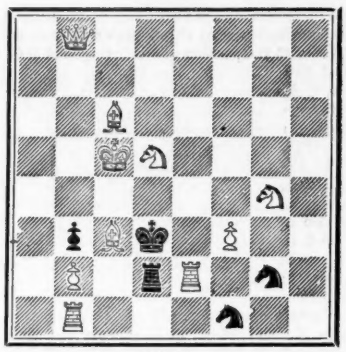

WHITE.

White to play and mate in two moves.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

W.—Many thanks for Problems. They are welcome. In the Problem referred to, the conditions were unintentionally omitted.

Correct solutions received from W., Caissa, George Gouge, A. W. Overton, W. Hanrahan, John Watson.

Notice to those of our readers conducting games by correspondence through the medium of KNOWLEDGE. We shall be glad to receive and publish for the purpose of criticism any position of general interest requiring analysis, either in the opening, middle, or end game.

ERRATUM.—In KNOWLEDGE No. 135, p. 393, second column, line 7, for "tons" read "foot-tons."

CONTENTS OF No. 136.

	PAGE
The Movements of a Giant Sun. By Richard A. Proctor	405
The Chemistry of Cookery. XXXV. By W. M. Williams	406
Double Stars. (*Illus.*) By W. S. Burnham	408
Patent Act of 1883. By Polyglot	409
Sent to the Bottom. By Richard A. Proctor	411
The Amateur Electrician. (*Illus.*)	411
Foucault's Pendulum Experiments. (*Illus.*) By R. A. Proctor	413
The Entomology of a Pond. (*Illus.*) By E. A. Butler	414
International Health Exhibition. (*Illus.*) By J. E. Ady	415
Spider-Life Wonders	418
Recent Extraordinary Sunrises and Sunsets. By Captain Noble	418
The Dynamite Outrages. By R. A. Proctor	418
Editorial Gossip	419
Reviews	420
The Face of the Sky. By F.R.A.S.	421
Correspondence	421
Our Mathematical Column: Easy Lessons in Co-ordinate Geometry.	425
Our Chess Column	426

SPECIAL NOTICE.

Part XXXI. (May, 1884), now ready, price 1s. 3d., post-free, 1s. 6d.

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5. COMETS.
6. THE STAR DEPTHS.

See Advt. Pages for full Syllabus.

The following arrangements are complete: the numbers in brackets referring to above list.

NOTTINGHAM, June 18, 19 (3, 4).

NOTE.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*